NEW-YORK
MEDICAL AND PHYSICAL
JOURNAL.

VOL. VI.] APRIL, MAY, AND JUNE, 1827. [No. II.

ART. I. *Remarks on Delirium Tremens, or the Irritative Fever of Drunkenness.* By CHARLES STUART TRIPLER, M. D. of New-York.

WHEN persons, who have long indulged in the excessive use of ardent spirits, are suddenly deprived of their accustomed stimulus, they frequently become the subjects of a peculiar and interesting disease. To this affection, the various appellations of "Delirium Tremens," "Mania a Potu," "Mania a Temulentia," and the "Brain Fever of Drunkenness," have been given by the different authors who have noticed it.

It is but a short period since the attention of physicians has been in any degree particularly directed to this complaint. In the text books of the student it is not even mentioned; and most that is to be learnt concerning it, must be searched for through our periodical works. Dr. Armstrong, in his excellent work on Typhus, has given a very concise and correct history of the disorder, together with its symptoms and mode of treatment. He thinks that Hippocrates has described it in his Epidemics, particularly in the case of Chærion, in the third book. It appears, however, that among the moderns, Dr. Samuel Burton Pearson published the first account of it. In 1812, a short paper on the subject, by Dr. Armstrong, and in the year following, the "Tracts" of Dr. Sutton, were published. Since that time, a number of papers, and cases of the

disease, have appeared in different periodical journals, and more particularly in the United States. The most prominent of these are the communications of Dr. Klapp, published in the seventh volume of the Eclectic Repertory, and in the first volume of the Medical Recorder; the cases of Dr. Channing, in the eighth volume of the New-England Journal of Medicine and Surgery; and the "Observations on Delirium Tremens," by Dr. Brown, in the fifth volume of the Medical Recorder.

Having had occasion to treat a number of these cases, during a short residence at the New-York Almshouse and Penitentiary, as House Surgeon to those institutions, I have thought proper to make it the subject of the following remarks.

The disease naturally divides itself into three stages:—1st, Oppression; 2nd, Excitement; 3rd, Collapse.

First. The stage of oppression is marked by lassitude, indistinct chills, oppression at the præcordia, loss of appetite, nausea, and sometimes vomiting. In this stage the patient is somewhat inclined to sleep; but the drowsiness soon leaves him, and in a short time the second stage commences.

Second. In the stage of excitement, the tongue is furred, but moist; the bowels usually costive; the pulse full, and generally more frequent than natural; the skin in most cases hot, but moist; the eyes are turgid, wild in expression, and quick in their motions; the hands and knees become affected with a peculiar trembling; the urine is high-coloured, and small in quantity. The patient talks incoherently, occasionally starting as if from some frightful object; shows no disposition to lie down, and is utterly unable to compose himself to rest. He imagines that he is unrelentingly persecuted by some enemy, for a crime of which he is innocent; that every cup presented to him contains poison; that he sees the devil springing upon him, or some ugly animal running over his bed or about the room; that his business is deranged; that he is swindled by his debtors; with many other equally absurd and unlikely incidents. I had one patient, who imagined a host of fiends were burning her to death with gas lights. These patients are very impatient of contradiction,

extremely obstinate in the belief of their hallucinations, and can be managed only by the mildest, most soothing and persuasive treatment. These symptoms undergo an exacerbation in the evening, and a remission towards morning.

If, now, the disease be suffered to continue, the *third* stage, or the stage of collapse, comes on. The pulse becomes more rapid and small; the pupils are contracted; the tremor is increased; the tongue and jaws have a trembling motion; the greatest anxiety is depicted in the countenance; the skin becomes cold, and is covered with a peculiar cold and clammy sweat; convulsions, coma, and subsultus tendinum ensue, and death soon closes the scene.

In the fatal cases which I have witnessed, the unfortunate result has uniformly been preceded by a suppression of urine, for twelve to thirty-six hours.

The following cases, with their treatment, may serve to illustrate our observations.

CASE I.

R—— M——, aged fifty-two, was committed to the Penitentiary as a vagrant, June 3rd, 1826. When I saw him, he was labouring under an epileptic fit, foaming at the mouth, turgid face, and breathing hard. He was immediately bled from the temporal artery about 20 ounces: he then began to tremble, and the nature of the disease was at once manifested. Being impressed with the idea that opium was a specific in this disease, and in all its stages, I immediately entered upon its use in this case. ʒij. tr. opii were administered at once, and ʒj. every half hour prescribed, until he should fall asleep. Upon visiting him at 9 P. M., I found to my surprise that he had exhibited no signs of sleep, although ℥j. tr. opii had been given him. He was sitting upon his nates, with his legs drawn up, and his head resting upon his knees. He was then ordered gr. ij. opii every half hour. In the morning of the next day, I found that he had slept none since his admission. The use of the tr. opii was then resumed. In the evening found him more sensible, but still remarkably stupid and obstinate, occasionally raising his head, and looking anxiously

at his pillow. Having been sadly disappointed in the anticipated effects of my specific, I now determined to try the cold affusion, so highly recommended by Armstrong. He was accordingly stripped, and two pailsful of water were poured moderately over his head and trunk; the laudanum was then continued. At 9 P. M., found that he still slept only at short intervals: had not made water since his admission. The catheter was introduced without difficulty, and only eight ounces of water were drawn off. The cold affusion was then repeated, and the tr. opii ordered to be continued.

June 5th, A. M. Found him more rational; would talk a little, and said he should soon be better; had passed no more urine, and had slept but little during the night: has had no stool since he came in. Ordered the common house clysters to be repeated till they should operate. The third produced slight evacuations. The man died on the fourth day from his admission. At the commencement his pulse was full, but became more rapid and smaller immediately after the bleeding, and a profuse sweat made its appearance over his body. The cold affusion was the only remedy that appeared to moderate his delirium or to cool his skin; but these effects were merely transitory.

CASE II.

June 8th, W——, committed last evening as a vagrant. Found him this morning labouring under delirium tremens, with a dry skin, flushed countenance, full and soft pulse. Ascribing the unfortunate result of the preceding case to my own inexperience and injudicious arrangement, in not having properly prepared the patient for the use of the opium, I requested Dr. Brown, Consulting Surgeon of the establishment, to visit this man with me. By his advice, I bled him about 10 ounces, which brought out the sweat, and increased the rapidity of his pulse. He was then put upon the use of tart. ant. et potass. gr. one quarter, in solution, every two hours. At 3 P. M. I found the man's pulse the same as before bleeding. He had raised a considerable quantity of bile, without much nausea. Continued the antimonial solution.

June 9th. Pulse rather more frequent, but softer. Antimony operating on the bowels. Patient had slept well during the night.

June 12th. Discharged from the Hospital, cured.

This, though by no means so severe a case as the preceding one, was still replete with satisfaction and instruction to me. Had I given him opium at his admission, he had no doubt shared the same fate.

CASE III.

L——, committed as a vagrant on Saturday, June 10th. Found him labouring under delirium tremens, but not very severe. Pulse not much increased in frequency, but rather fuller than natural. He was ordered one quarter gr. tart. ant. every two hours. *Sunday.* Medicine had operated once by vomiting, and several times by bilious dejections. *Monday.* Reduced the dose of antimony to one-sixth gr. every two hours. *Tuesday.* Found he had slept well last night; bowels costive. Ordered calomel gr. xx., rhei gr. xv., ant. tart. gr. j. *Wednesday.* Medicine had operated well. Patient slept well last night; and was discharged cured.

CASE IV.

H——, committed as a vagrant on Tuesday, 13th June. Came in with delirium tremens; pulse 80 in a minute, and rather full; trembling of the hands; no appetite; foul tongue. Ordered sulph. sodæ ℥ij., ant. tart. gr. ij., one half to be taken immediately, and the remainder three hours after. *Wednesday.* Medicine had operated well; the first dose as an emetic, and the second as a cathartic. Tongue still foul. Towards evening his tremor and delirium had increased, his pulse had become more rapid, and the peculiar sweat appeared on his forehead. The temperature of his skin was natural. Had slept none since his confinement. I gave him ʒiij. tr. opii at 6 P. M., ʒij. with a pint of warm beer at 9, and ordered ʒij. to begiven at 12, in case he should not previously fall asleep. *Thursday.* Found he had slept none last night; pulse more rapid; skin hotter; delirium increased: had not passed urine

since yesterday at 11 A. M. At 3 P. M., 20 grs. calomel with 1½ gr. ant. tart. were administered. At 9 P. M., medicine had not operated. Ordered ʒiſs. ol. ricini to be taken immediately, and ʒij. tr. opii at 12 o'clock, with directions that ʒj. should be given every two hours after, until he should fall asleep. *Friday.* Found the nurse had mistaken my directions, and had given ʒj. tr. opii every hour. The man was decidedly worse; had slept none; bowels had not been moved; had made no water since last report; eyes turgid; tongue much furred. The man died at 12, noon.

Dissection. The dura mater was found in a congested state; the tunica arachnoidea much thickened, opaque, and watery effusion between it and the pia mater. The veins of the pia mater and brain were highly congested; the lateral ventricles greatly distended with water. The stomach was natural, much contracted, and empty; the liver hard, and of an ash colour; the gall-bladder filled with bile of the consistence of tar; the omentum smaller than usual, and adherent to the small intestines. The bladder contained but half a pint of urine: the kidneys were natural.

CASE V.

B——, aged thirty-six, committed as a vagrant August 1st, 1826. Found him labouring under symptoms of approaching delirium tremens, with a full pulse, foul tongue, and contracted pupil. I immediately prescribed for him tart. ant. gr. x. dissolved in water ʒiv., one table-spoonful every ten minutes. The medicine vomited and purged him freely. In the evening he was ordered camphor and opii aa. gr. ij., ant. tart. gr. one-sixth, in the form of pill, every two hours. *Wednesday, 2nd,* A. M. B. had vomited the pills, and could retain nothing on his stomach. His pulse was smaller and more frequent, and his pupils much contracted. Towards evening he retained two of the pills upon his stomach. I then gave him gtt. xl. of the black drop, with orders to have it repeated every two hours, with a small quantity of brandy and water, until he should sleep. *Thursday,* A. M. Found he had taken 160 drops of the medicine, with no effect. Towards evening

he became so furious, that it was necessary to confine him alone in a cell. I now gave him 90 drops of the black drop at once, and in two hours after 40 drops more, with directions to have the latter quantity repeated every two hours. *Friday*, A. M. Found he had taken but 60 drops since I left him, and none since two this morning: had slept none during the night. I gave him 90 drops at 7 A. M. and 40 at 9. He laid down in a few moments, appeared to be comatose, pulse small, a cold clammy sweat over his body, and apparently hastening to the grave. In the course of four hours he rose up as furious as ever, and refused to take any more medicine. He, however, now asked for some beer, of which I took advantage, and gave him one pint, containing 60 drops of the black drop. He swallowed it, laid down, and slept soundly from 6 P. M. until midnight. He then began to hollow again, but soon became more calm and sensible. I had directed his keeper to give him 40 drops more of the black drop, in case he should wake before 10 P. M. He thought, upon visiting him in the morning, that if he could have required it then, it would do as well now, and administered the dose. The patient then fell asleep again, and continued so till 10 A. M. When he awoke, he was perfectly sensible, but complained very much of thirst. Some beer was given him; the first draught of which was immediately rejected, the second he retained, and declared quite refreshing. During the afternoon he was allowed whiskey and water to drink. Towards midnight he again began to cry "murder!" and "water!" and in the morning was quite exhausted, complaining of a most intolerable thirst. He had eaten nothing for the last sixty hours. Toast and tea were given him for breakfast, which he appeared to relish very well. From this time he recovered rapidly, and required no more medicine.

This was the most interesting case I ever witnessed. The quantity of opium taken is surprising; but the result justifies the treatment. In the management of this case, I was much indebted to Isaac Wood, M.D. (Consulting Physician of the institution at that time) for his advice and support.

Treatment. The diversity of opinion existing among authors, with regard to the condition of the system in this complaint, has necessarily led to the proposal of very different methods for its relief. A high degree of nervous irritation, and a disturbed state of the equilibrium of the circulation, seem to exist throughout the disease: still the habits, age, temperament, &c. of the patient so modify its appearances, that no one course of treatment can be laid down to suit every case. In some cases, the symptoms of oppression are immediately succeeded by those of collapse, without any intermediate signs of excitement. In others, the excitement rises to such a degree, that the disease might be confounded with phrenitis. And indeed this is often the case with young subjects: but should the means proper for phrenitis be employed, the subsequent prostration would soon convince the practitioner of his error. It is therefore necessary, that in every case a correct history of the disease should be obtained, before the physician can with confidence enter on any course of treatment. To a neglect of this precaution, either from inattention, or from the difficulty of obtaining the necessary information, may perhaps be attributed the failure of remedies which, under other circumstances, might have proved successful.

The means of cure suggested by authors, are principally the following:——Blood-letting—purgatives—emetics—cold and tepid affusions, and diffusible stimulants. We propose to examine the merits of each somewhat in detail.

Blood-letting is much insisted on by Professor Potter of Baltimore, in this disease; and indeed the principal fault he finds with Dr. Armstrong's method of treatment, is his want of confidence in this remedy. He remarks, that he has frequently drawn even an hundred ounces of blood from the arm in three or four days; and also, that "the menacing character of the symptoms in the first stage, either in the forms of long-continued convulsions, sometimes indicating approaching apoplexy, or the presence of a ferocious delirium, have compelled us to draw 20 or 30 ounces of blood, without removing the ligature from the arm." Still he admits, that "so sanguinary a procedure" is

seldom necessary, and not often admissible, though he has never had cause to regret it. I never bled a patient without regretting it. True, I seldom met with any but habitual drunkards, which undoubtedly makes an essential difference: but the rapid increase in the frequency of the pulse—the immediate appearance of the clammy sweat—the aggravated trembling, and great prostration of strength consequent to the subtraction of a very small quantity of blood, in those cases in which I have tried the remedy,—have induced me to determine on never employing it again, except in a first attack of the disease, or in a young and very plethoric subject. The weight of evidence is decidedly against the practice. Dr. Sutton observes, that he has "witnessed the cases of this disease to be always most rapidly fatal in robust and plethoric persons, even where blood-letting was much used without the aid of opium."* And in another place, as quoted by Dr. Channing: "The tremors are rather a symptom of a peculiar disease, which has hitherto been considered to be phrenitis, and when treated as such, has proved to be fatal." "When blood-letting has been employed, and principally relied on, I have observed a fatal termination of the disease in almost every case, though the indication as to habit for its use appeared strong and decisive." Again: "On the contrary, when in such habits," (robust and plethoric,) "the delirium tremens has been endeavoured to be overcome by opium: the result has been the most favourable, because the constitution is less broken down." "In a very early stage of the paroxysm, however, if in a very plethoric subject, blood may be drawn; but I do not recommend the practitioner to wait to see the effects of it, with a view of repeating the operation, as thereby the life of the patient may be endangered." Dr. Channing bled his first patient while the perspiration was profuse: he was also vesicated, purged, and had anodynes at night. He became comatose on the third day, was attacked with convulsions, and died. Four other cases of his were

---

* Medical Magazine.

attacked with pulmonary inflammation, and treated in the usual way. They were attacked with delirium tremens after the other symptoms were moderated, and two of them died: the other two were cured by calomel and opium. Dr. Armstrong says he invariably saw blood-letting prejudicial, even at the onset, in debilitated and habitual drunkards: but "in constitutions that have not been shaken by reiterated drunkenness," he remarks, "I have known early and moderate venesection of much use, especially when followed by active aperients." Dr. Brown observes: "Our experience constrains us to coincide in opinion with Dr. Armstrong, in regard to the use of the lancet in this disease." Again he remarks: "Even in cases where convulsions come on early, emetics will generally have a more decided and happy effect than the abstraction of large quantities of blood." The fulness of the pulse in the early stages of the disease, is most probably owing to irritation, and not to inflammatory action. It is entirely devoid of strength, and is easily compressed. All authors agree that bleeding is inadmissible, in the latter stages, in organic derangements, and when the system has been worn out by repeated attacks.

Purgatives. The use of purgatives promises more benefit than that of the lancet; but still they are in no severe case to be exclusively relied upon. "I am fully persuaded," says Armstrong, "that there are not many instances where the use of the lancet is really requisite; and also, that there are few where purgatives should be omitted in the commencement." I have observed that in most cases the bowels are disposed to be torpid, and have always found early purging highly beneficial. In fact, numerous instances have occurred in the Bellevue establishments, in which nothing else was done but to give the patient a purge, and shut him up in a dark room. These were of course mild cases; but still they show the good effects of cathartics. It is also a fact, that when the bowels are constipated, opium will not have so good an effect as when they are in a soluble state. And it is extremely unpleasant to be obliged to stop the exhibition of opium at a late period, for the purpose of interposing purgatives. The most proper

time for their employment, particularly in habitual drunkards, is evidently in the commencement of the disease, before the stage of collapse shall have supervened: after this period, active purging would be prejudicial. Dr. Armstrong very justly remarks, that after four or five days, laxative enemata will be found most proper.

Emetics next claim our attention. Dr. Klapp of Philadelphia, who has written on this disease, and published some cases, from peculiar notions of its pathology, depends chiefly on emetics. He indeed seems to regard them as almost specifics; but his recommendation must be received with some qualification. They are certainly in some cases, and in some stages of the disease, a highly valuable remedy; but in other cases, and under other circumstances, they are as certainly prejudicial. Hepatic obstructions and congestions are of frequent occurrence, in those who indulge freely in the use of ardent and stimulating liquors: these are undoubtedly best removed by emetics. But in their exhibition, reference must be had to the habits of the patient, whether an habitual or an occasional drunkard, and particularly to the stage of the disease. In the early stages, when there is much nausea, a foul tongue, a bad taste in the mouth, with loathing of food, and oppression at the præcordia, an emetic will be found highly serviceable, and sometimes indispensable. I have seen instances in which the patient has recovered, without its having become necessary to use any other means. This effect, however, I do not attribute merely to their action in relieving the stomach of crapulous matter; that, undoubtedly, has its share in keeping up the disease, and by its ejectment one source of irritation is got rid of. The efficacy of the tartrate of antimony and potash, (which is the emetic recommended by Dr. Klapp,) is more probably owing to its well ascertained effects in allaying nervous irritation, and equalizing the circulation, thus inducing sleep, which, once obtained, generally places the patient beyond danger. The best emetic, therefore, which can be employed, is the tartarized antimony. Where there is much excitement, the following will be found an excellent prescription:

℞. Tartarized Antimony, gr. ij.
Distilled water, - - ℥iv.

Dissolve, and give one table-spoonful every two hours. The first dose will generally vomit, and the remainder purge the patient. When, however, it is desirable to procure full vomiting, the medicine must be given more frequently, and in larger quantities. But the use of this remedy is not to be confined merely to its emetic properties. That excitement for which the lancet has been so freely recommended, will, in most instances, be found to be more easily and permanently reduced, and with less hazard to the patient, by the vicarious employment of antimony. Given in small doses, it equalizes the circulation, reduces morbid excitement, allays nervous irritation, and, if it do not of itself induce sleep, it prepares the system in the best manner for the use of opium.

Affusions. With regard to the use of the cold affusion, I can say very little; and of the tepid, nothing from my own experience: but having the sanction and recommendation of Armstrong, there can be no doubt of their importance in this disease. I made use of the cold affusion in three cases, and apparently with good effect; but I did not persevere sufficiently in its application to judge of its merits.

Diffusible Stimulants; and, first, of Opium. This we consider to be the most important remedy in the treatment of delirium tremens. After every other means have failed, this is the great sheet-anchor of our hopes. Seldom, when it has been judiciously employed, have we been disappointed in the result; never have we had cause to regret its exhibition. True, it may be, and has been, exhibited in cases where it was directly contra-indicated: but the unfortunate result of these, can surely furnish no conclusive evidence against the utility of this valuable medicine. Led away by the most absurd and extravagant notions of its specific efficacy in every case, and in all conditions of the system, in the first cases which fell under my care, I employed it under circumstances and to an extent wholly unwarrantable. But the unfortunate result of my practice soon taught me the truth of the maxim—

"Est modus in rebus; sunt certi denique fines,
Quos ultra, citraque, nequit consistere rectum."

Opium is, in its primary operation, a highly diffusible stimulant, and to justify its employment, there must be a degree of prostration: but if given when there is much excitement, it only increases that excitement, together with the congestions, to which there is always a strong disposition in this disease. Hence probably have arisen the prejudices entertained against its employment, by so many respectable physicians. To suppose that opium will answer every indication, in all stages of the disease, is not only visionary, but idle and hazardous in the extreme. It is necessary that the system should be properly prepared for its exhibition before it is commenced with. In the early stages of the disease, there is usually a considerable degree of excitement, with heat of skin, and great restlessness. This is the proper time for the use of antimony and purgatives; and opium given at this period, would be productive of more injury than benefit. But after the stage of excitement shall have passed over, when the pulse has become small and frequent, the skin cool, the surface of the body covered with the cold sweat, the limbs violently agitated, and no sleep has been procured for some time,—then is the time for giving opium. If any benefit is to be expected from it, it must now be given faithfully and liberally. Dr. Armstrong observes, that "under the most unpromising circumstances, a combination of calomel and opium will *sometimes* succeed; and whenever there is ground for doubting the propriety of evacuations, it should be administered in preference to any other expedient." As to the manner of exhibiting this combination, he remarks: "Two or three grains of calomel, with a grain and a half of opium, every six or eight hours, will be sufficient doses of these medicines on the first day of their administration; and after that period, it will be better commonly to lessen the quantity of opium: and as soon as the action of the calomel is at all developed on the gums, or salivary glands, it should be entirely omitted, as its effects for the most part continue to increase for a few days afterward." This, I am compelled to say, from what I have seen of the disease, appears to me to be but temporising treatment. The learned author gave the calomel to equalise the

circulation, probably, as Dr. Brown has remarked, from his pre-conceived notions of the existence of venous congestions as the proximate cause of the disease. These congestions, as is evident from the nature of the symptoms, do not exist, other than as a consequence of the disease thus constituting the pathological condition of the last stage. The object, therefore, in the treatment of the disease, is to prevent this state of things, and not to cure it before it exists. We have before remarked, that the state of the pulse in this disease, is most probably owing to irritation. If this position be true, the most rational means of cure will be those whose operation it is to allay this irritation in the most direct and speedy manner. It is in this manner, in my opinion, that opium and antimony cure the disease—the supervention of sleep being merely an evidence that the desired effect has been produced.

Professor Potter, in his note on Dr. Armstrong's paper, has given some valuable observations and facts on the use of opium in large doses; and has also pointed out the condition of system in which it is proper it should be given. "The use of opium *alone*," says he, "in certain cases of this disease, is clearly indicated, when it has been protracted till the pulses become weak and very frequent, tremors very great, and the state of the understanding indicates a loss of energy in every part of its empire. In such cases, large doses have produced effects so extraordinary, that some have been induced to prescribe them to the very great injury of the patients." This accords entirely with my observations; and I must attribute Dr. Armstrong's coyness, in regard to the exhibition of opium alone, and in large doses, to the fact that "he has seen and heard enough, to be fully convinced that it is a very perilous practice to administer it in too large and repeated doses, since apoplexy, coma, and convulsions, may be thereby produced." I am well persuaded that too many cautions cannot be given, nor too close an examination of the symptoms recommended, previous to the exhibition of this drug. But I am equally well persuaded, that when it has been once clearly and satisfactorily ascertained that opium is indicated, large doses will be found of most benefit. By large doses, I do not wish to be

understood as recommending opium to be given by the scruple, or laudanum by the ounce, and to all patients alike. The term "dose" is relative, and must be considered relatively, to the condition of each patient. Thus the habitual drunkard, and one who has long been accustomed to the use of inordinate quantities of liquor, will both bear and require larger doses than the comparatively youthful debauchee. And again; larger doses will be required, in proportion as the case has verged nearer a fatal termination before advice has been sent for.

By referring to my last case, it will be seen that preparations of opium, amounting to at least five ounces of laudanum, were given in the course of five days, with success. My preceptor, Dr. Brown, (to whom I am indebted for the first correct notions I ever entertained on this subject,) has recorded a case, in the third volume of the New-York Medical and Physical Journal, in which 40 grains of opium were given in four hours, successfully. In Dr. Channing's last case, 20 grains of opium were administered in twenty-four hours; 12 of which were given in the two hours immediately preceding sleep, and with a successful result.

In addition to the above, I may here briefly detail two interesting cases which came under my own observation. The first was a man nearly fifty years of age, of a sanguine temperament, and an habitual drunkard. He came into the Penitentiary in a confirmed state of delirium tremens, and in the last stage. His pulse was extremely rapid—his surface was covered with a cold sweat—his eye suffused, and remarkably wild—pupils contracted—tremor of the hands amounting almost to subsultus; and was, upon the whole, one of the worst cases I ever saw. One hundred and fifty drops of the black drop were immediately administered in a half pint of brandy and water, made of equal parts of each. I then directed 40 drops more, in a like quantity of brandy and water, to be given in two hours, if the first dose should not have produced sleep previous to that time. The first dose, however, did produce sleep; but, to save himself the trouble of watch-

ing the man, the nurse woke him up, and gave him the second dose. The man slept sixteen hours, and awoke perfectly sensible.

The other case was a boy of sixteen, admitted at the same time, and in nearly the same condition. Ninety drops of the black drop were given him in brandy and water, which had the effect of producing a sound sleep for twelve hours. The next day some cathartic medicine was administered, and he rapidly recovered.

The practice of giving opium in this disease, is as old as the description of the disease itself; but the credit of having brought it to its present perfection, is unquestionably due to Dr. Brown, whose writings on this subject we have so often referred to. The great leading object, he remarks, in the treatment of this disease, is to procure sleep. This effect is most certainly obtained by the exhibition of opium. It must, however, be given with an unsparing and judicious hand.

In some cases, it is no easy matter to persuade the patient to take his medicine. In these, I have often succeeded by smuggling the laudanum or black drop into a glass of brandy and water, and then inviting the patient to take a glass of grog. By this means the idea of medicine is not presented to the mind, and the dose is swallowed without difficulty.

The other diffusible stimulants, which will occasionally be found useful in this disease, are beer, porter, gin, and brandy. If they can be given warm, or in the form of toddy, they will produce a better effect than when given cold. I once succeeded, in a very desperate case, in procuring sleep, by persuading the patient to take about a pint of warm gin and porter. These liquors also afford an excellent vehicle for the administration of the preparations of opium. With respect to the further exhibition of these stimulants, during convalescence, it appears to me entirely unnecessary, and may have some effect in leading to a re-indulgence in their use. If the debility be very great, a mild course of tonics, with an occasional laxative, will be found far more advantageous.

Theory of the Disease. Health consists in the harmonious performance of all the functions. Some of these functions

are essential to life, and hence have been called vital. Of these, the most prominent are the nervous, the vascular, and the respiratory. These functions are severally capable of acting and re-acting upon each other. The natural stimulus of the heart and arteries is the nervous energy; the natural stimulus of the brain is the arterial blood; and neither of these could exist without the function of respiration. When all these functions are strictly balanced, the other functions of the human body are generally performed as they should be, and health is the result of this harmony. But when, by the action of morbid influences, the vital functions are deranged, the animal functions soon follow, and disease and its phenomena are presented. This derangement may consist either in prostration or excitement, thus varying the character of the disease. Nervous energy may be increased, either directly, by the action of stimuli on the stomach, or indirectly, by their action taking place through the medium of the vascular system. By the gradual application of direct stimuli, the nervous energy may be so gradually increased, as to produce no very sensible effect upon the vascular system; and it may at length become even necessary, to the production of a sufficiency of this principle to enable the heart and arteries to perform their functions. This is well illustrated in the case of the dram drinker. So long as he continues his potations, so long he feels right; but the moment he stops feeding the fire, which, although consuming him, has now become, in a measure, necessary to his existence, a diminution of nervous energy and prostration is the result. Still the spark of life is not quite exhausted, as is shown by a re-action occurring shortly after, with excitement. It is this state which Mr. Travers has denominated *prostration with excitement.* He remarks: "Re-action may be gradual, and restorative of a natural and tranquil state of the system, as exemplified in an ordinary fainting fit; or it may be irregular and violent; in fact, an action unsupported by sufficient power to maintain and carry it through; and in this case the state of exhaustion into which the patient lapses, quickly terminates life." This is strikingly analogous to the disease we are considering.

Some patients get well without any treatment, or with very little, and are examples of the first kind: others require the removal of every source of irritation, together with narcotics, which allay existing irritation, thus moderating excitement, and are examples of the second kind of re-action. It is also remarkable, that the treatment proposed by Mr. Travers for his cases, (arising from surgical causes,) is the very one we have always found most beneficial in this disease; thus adding to the probability of our conclusion, that the condition of the system in both cases, is cæteris paribus, precisely similar.

If this view of the subject be correct, the disease may be properly called "The Irritative Fever of Drunkenness."

ART. II. *The Surgical Anatomy of the Iliac Fossa. Translated and abridged from the Surgical Anatomy of* VELPEAU. By JOHN W. STERLING, M. D. of New-York.

[BESIDES the two grand divisions of anatomy into general and special, or descriptive, a third branch, and which may be considered as the complement of the two others, has recently engaged the attention of the zealous cultivators of this science in both hemispheres. This is, the Anatomy of Regions, or, as it is often called, from its obvious importance in surgery, Surgical Anatomy. Every practical surgeon must, of course, have studied the relations of parts, as preparatory to the performance of particular operations, but as a regular part of a surgical education, surgical anatomy has only been taught within a very few years; and though many valuable monographs have been written by Camper, Cooper, Lawrence, Soemmering, Beclard and others, nothing like a systematic treatise on this subject has been attempted. The works of Colles, and Burns, are valuable as essays or sketches on particular points of surgical anatomy; but they are far from being complete, even on the few subjects they profess to treat of. Much valuable information is scattered through other works of less merit, and which are more or less generally known in this country; but nothing

like a regular treatise, in fact, on the anatomy of the various regions, has been presented to the profession, until within a few months since, when the work of M. Velpeau made its appearance in France. This is a manual for students in dissections, in reference to surgical operations and diseases, and a book of reference for the operating surgeon. The human body is divided into regions by natural and artificial lines, and the parts are described in the order of super-position as nearly as possible from the surface to the internal part.

The zeal and ardour with which the improvement and diffusion of medical science are promoted since the recent organization of the College of Physicians and Surgeons, are shown in this early attempt to introduce among us the fruits of the industry and talents of a very able anatomist of the Parisian school; and we are happy to bear testimony to the ability with which Dr. Sterling is executing the translation, if indeed that do not deserve a better name, which consists in the abridgment of a work in a foreign language, and the correction of some anatomical errors.

The following specimen of the work is selected, because it does not, like some of the more interesting parts of the work, require plates for its illustration.—Ed.]

Art. III. Section V.

OF THE ILIAC FOSSA.

As this portion of the abdominal cavity appertains both to the posterior and lateral parietes, but does not belong exclusively to either, we make it a distinct section. We have separated it from the pelvis, properly so called, because it is too intimately connected with the iliac region already described, and especially because it will permit us afterwards to pass in review the greater part of the abdominal viscera.

This fossa is bounded, superiorly, by the spine of the os ilii, where it presents a slight groove, which is deepest at its posterior part;—inferiorly, by the posterior margin of Poupart's ligament; and internally, by the inner border of the psoæ muscles, or, in other words, by a line drawn from the

sacro-vertebral angle to the spine of the os pubis. In its two external thirds we observe a kind of excavation, fossa, or gutter, much deeper in the female than the male, which gradually becomes narrower as it descends and terminates in a slight cul-de-sac, or rather groove, which extends from the spine of the ilium to the external side of the iliac artery, where this vessel is about to pass under Poupart's ligament. This groove is, below the crural arch, what the external inguinal depression is above it: we will therefore call it the *external iliac fossette.* At the inner part of the iliac fossa there is a prominence, which is very large near the sacro-vertebral angle, but afterwards decreases until it reaches the crural arch, where it limits the groove just mentioned, and within which we observe another groove, bounded, anteriorly, by Poupart's ligament; posteriorly, by the linea ilio-pectinea; externally, by the epigastric and external iliac arteries; and internally, by the origin of the umbilical ligament. This excavation, which we will name the *internal iliac fossette,* is below the middle inguinal fossette, and corresponds to the superior orifice of the crural canal. The prominence just mentioned, borders the brim of the pelvis, and separates the cavity of this basin from the iliac fossa, properly so called: on the right, this fossa is filled by the cœcum; on the left, by the sigmoid flexure of the colon.

CONSTITUENT PARTS.

1st. *The Peritoneum.*

This membrane is very supple in the iliac fossa, and can be easily separated from the parts which it covers. It appears thinner than in the other regions, because the cellular layer which invests it is detached from it: it is adherent only where it approximates the spine of the ilium, and is thereby well disposed to form an envelope for the viscera, when they protrude through the natural apertures of the abdomen.

2nd. *The Fascia Propria.*

Beneath the peritoneum we invariably find a considerable

quantity of lamellated cellular texture, which, in the majority of subjects, forms a membrane sometimes very evidently fibrous, and which is, in reality, only a continuation of the subperitoneal cellular layer. If we commence with it in the middle of the iliac fossa, we will be able to trace it to the upper part of the spine of the os ilii, between the serous membrane and the transversalis muscle, but becoming more and more attenuated, and finally confounding itself with the fascia transversalis. Towards the loins, i. e., in ascending towards the kidneys, it is thicker and more supple, especially on the outer side of the psoas muscle. As far as the posterior surface of Poupart's ligament, between the spinous process of the ilium and the iliac vessels, it remains very distinct, and is only adherent to the peritoneum and fascia iliaca by means of a very extensible and supple lamellated tissue, in which we frequently meet with large and soft adipose vesicles. As it passes over the anterior surface of the psoas, its laminæ separate, to form a sheath for the spermatic vessels: it soon envelopes the ureter in the same manner, and likewise gives to the iliac vessels a complete tunic. When it arrives near the iliac fossettes, it gives off envelopes to the origins of the circumflex iliac, epigastric and umbilical arteries, and the vas deferens. At this place it may be easily divided into two sheets; one of which ascends with the peritoneum, and the vessels, upon the internal surface of the transversalis muscle; the other, which is thinner, lines the bottom of the internal iliac fossette, is prolonged into the crural canal, and is lost, on the one hand, in the fascia superficialis of the thigh, and on the other, is expanded upon the posterior surface of Gimbernat's and Poupart's ligaments, and is apparently continous above the latter with the fascia transversalis.

Until the present time, the fascia propria has scarcely been mentioned by anatomists. It is of importance, however, to understand it well, in order that we may comprehend in what manner it is related to the fascia transversalis, with which the sheet just spoken of has been frequently confounded; not that we ought to make of it a separate aponeurosis, for we think that these layers have already been sufficiently multiplied as

distinct organs; but we have not gone far enough in endeavouring to ascertain their continuity, and it is time to make some attempts for this purpose. Thus, is it not certain that all the aponeuroses are only modifications of the cellular tissue, and that we may derive them from a small number of centres? For example; we have already seen that all the fasciæ of the neck are continuous, by ultimate analysis, with the pericardium on the one part, and, on the other, with the aponeuroses of the arm, which, in their turn, terminate upon the scapulo-humeral articulation: we have also seen that the subcutaneous cellular layer every where formed a true fascia superficialis, which can only be regarded as an appendage to the fibrous element, with which it has numerous points of contact. With respect to the abdomen in particular, is not this superficial layer continuous, in a direct manner, with the subperitoneal sheet (of the fascia propria), by numerous openings, and especially by the inguinal and crural canals? Finally, in order to give a distinct idea of the fascia propria in few words, we may say that it is, between the peritoneum and the parietes of the abdomen, only a repetition of the fascia superficialis between the parts covered by this layer and the skin; sometimes thin, at others thick; throughout the greater part of its extent single, in some points bifoliated; here, adherent to and confounded with other laminæ—there, on the contrary, supple, distinct, and easily detached. In every point of the belly it varies in some one of its characters, but is always the same membrane; and it is by its means, on account of its continuity, that the fluid products of inflammation extend, and spread from one part to another, with the greatest facility, and so much the more so where this layer is less compact.

3rd. *The Iliac Aponeurosis, or Fascia Iliaca.*

We may say that this is a continuation of the very thin membrane heretofore described as originating from the ligamentum arcuatum and the fibrous arch which embraces the origin of the psoæ, and is spread out over the anterior surface of these muscles. This membrane becomes gradually thicker as it descends into the iliac fossa, where its fibres generally

run in a transverse direction. In this fossa it is strong, dense, and almost inextensible: near the spine of the ilium it splits into two layers, for the purpose of embracing the circumflex iliac artery. It is very adherent to the inner labium of this spine, where it is continuous with the fascia transversalis. Internally, upon the fleshy portion of the psoas, it is thinner, but increases in thickness upon its tendinous portion. When the psoas parvus exists, the fascia iliaca is confounded with its tendon, or rather, this tendon expands into the laminæ of this aponeurosis, and gives to it additional strength. It serves to bind down these muscles; is spread over them, and behind the iliac artery and vein; and is inserted into the brim of the pelvis, where it unites with the fascia pelvia. Inferiorly, it is thick also, and its external half is slightly raised where it is attached to the posterior border of Poupart's ligament, extending its insertion from the spine of the ilium to about eight lines on the outer side of the external iliac artery: it then abandons this ligament, and its internal half is inserted into the linea ilio-pectinea (crête pectinée), and is again inserted into Poupart's ligament before it reaches the spine of the pubis (épine du pubis). As this aponeurosis is about to pass under the crural vessels, it detaches one of its laminæ, which is spread over their anterior surface, thus forming a sheath for them, which is soon confounded with that derived from the fascia propria: in other terms, the posterior border of Poupart's ligament appears to expand, and gives rise to two membranes, one of which ascends, and is applied to the internal surface of the transversalis muscle, forming the fascia transversalis; whereas the other sinks down upon the iliac fossa, to constitute the fascia iliaca: but this must be understood to relate to the external portion of the crural arch only. Indeed we may say, that as Poupart's ligament is passing from the ilium to the pubis, it bifurcates about the middle of its length, in such a manner that the superior branch of this bifurcation, or Fallopius' ligament, properly so called, follows its primitive direction, and goes to the pubis, forming, by its superior border, the external pillar of the inguinal ring; whilst the lower border of its extremity, in attaching itself to the bone, expands,

and prolongs its attachment from the spine of the os pubis one inch outwards and backwards upon the linea ilio-pectinea (crête pectinée), constituting what is called Gimbernat's ligament. The inferior lamina of this bifurcation sinks down upon the psoas and iliacus muscles, and when it arrives near to the vessels, it passes behind them, and following the body of the pubis, rejoins the expansion of Poupart's ligament. In consequence of this arrangement, a broad elliptical opening is formed, which constitutes the abdominal aperture of the crural canal. In short, it appears that the iliac, and transverse aponeurosis, circumscribe a broad funnel,* wanting its internal half, the pipe of which is represented by the crural canal, and opens into the groin; so that the fascia iliaca is actually continuous with the deep layer of the fascia lata, as it passes upon the body of the pubis and the anterior surface of the pectinalis muscle. If we take the precaution to remove all the parts which fill the crural canal, in order to examine its superior orifice, we will find that this opening is formed, anteriorly, by the slightly concave margin of Poupart's ligament; posteriorly, by the iliac aponeurosis, and the deep layer of the fascia lata, just where these too membranes unite at the crest of the pubis or upon the anterior surface of the psoas and iliacus muscles. This latter half of its circumference is much more concave than the former: externally, it terminates by an acute angle, which results from the bifurcation of Poupart's ligament; finally, it is limited, internally, by a semi-lunar concave border, which appertains more especially to the expansion of the external pillar of the inguinal ring upon the linea ilio-pectinea, or what is called Gimbernat's ligament. This ligament so incorrectly described by Gimbernat in 1793, in a Treatise on Crural Herniæ,† is not, therefore, a particular organ possessing a separate existence, but only, as Mr. Boyer has said, an expansion, a species of reflected lamina of

---

* Colles, p. 63.

† Nuevo Metodo de operar de la Hern. &c. por D. A. Gembernat. Madrid, 1793.

Poupart's ligament. As it is necessary, however, to have a distinct idea of it, in order that we may comprehend its exact relations with crural herniæ, we will consider it as if insulated; and, in this case, it will represent a small triangular membrane, its anterior border continuous with the posterior margin of the external pillar of the inguinal ring, its posterior border inserted into the linea ilio-pectinea (crête pectinée), and its base presenting the concave margin which has just been spoken of: consequently, its apex will correspond to the spine of the pubis, and its broadest margin will form the internal boundary of the opening of the crural canal. This last border is generally free, thin and cutting, and a great many surgeons, since the time of Gimbernat, have advised its division in strangulated crural herniæ; but sometimes it is continuous with the two layers of the fascia lata, so that its superior surface is inclined downwards and outwards, forming a groove, which completes the funnel of the crural canal.

The transverse diameter of this opening is about two inches, frequently two inches and a half in the female: its greatest antero-posterior diameter, from ten lines to an inch, which gradually diminishes as it approximates its extremities. It is naturally divided by the crural vessels into two portions. The internal portion is the largest, and has received the name of the *crural ring*. It is triangular, wider externally than internally; bounded on the pubic side by the concave cutting edge of Gimbernat's ligament; anteriorly, by Poupart's ligament; posteriorly, by the linea ilio-pectinea; and externally, by the crural vein, the external iliac, and epigastric arteries. It generally encloses a lymphatic gland, a prolongation of the fascia propria, and some adipose vesicles, which almost completely block it up: it gives passage to the viscera in crural herniæ. The external portion is triangular also, but much narrower, and is bounded, anteriorly, by Poupart's ligament; posteriorly, by the fascia iliaca, where, becoming continuous with the fascia lata, it is about to pass into the thigh upon the anterior surface of the iliac and psoas muscles; externally, by the angle formed by the union of the iliac aponeurosis with Poupart's ligament; and internally, by the femoral and epigastric

arteries. Most anatomists, in describing the crural canal, have neglected this portion, confining their attention solely to that which is on the pubic side of the vessels. It is true that it possesses much less interest, because it has not been proved that herniæ ever escape through it. We will indeed find, when we come to describe the inguinal region, that the anatomical disposition of the parts satisfactorily accounts for the situation of the displaced viscera before, and even external to the crural vessels, notwithstanding they have actually escaped from the abdomen, by passing between the crural vein and the base of Gimbernat's ligament. However, previous to Scarpa's day, it was generally admitted that the organs might protrude either before, external to, or on the pubic side of the artery; or rather, it was stated, in an indefinite manner, that they might glide between the vessels and the crural arch. At present, no person participates in this error: but has not Scarpa gone too far, in saying that external femoral hernia is impossible? We have frequently examined this portion of the crural canal upon the dead body, and have always found that, by sliding the extremity of the finger from without inwards, along the posterior surface of Poupart's ligament, that it would fall into the external iliac fossette, i. e., on the external side of the artery, so that we might, with a gentle effort, push it into the crural canal along the iliac side of these vessels. It must be admitted, however, that hernia is of very difficult occurrence at this point: 1st, because the excavation which leads to it is very superficial; 2nd, because the space is extremely narrow; 3rd, because the viscera will slip into the internal fossette sooner than separate and drive inwards the femoral artery. But sometimes the epigastric artery forms a greater projection than usual in the iliac fossa, and thereby increases the depth of the external iliac fossette so much, that the displacement might, at least, commence externally; and the possibility of such a disposition is worthy of note, for it is evident that, in a case of this nature, in cutting the stricture inwards, we would inevitably divide the epigastric artery, which would then surround the internal half of the circumference of the neck of the sac. The internal iliac fossette is,

on the contrary, perfectly disposed to admit of the escape of the viscera: 1st, because it is much deeper and broader; 2nd, because between the femoral vein and the semi-lunar edge of Gimbernat's ligament a space exists, which will readily admit the thumb, and even a larger body—a space which is filled by cellular tissue, and sometimes a lymphatic gland, substances affording but a very feeble resistance; 3rd, because, when the hernia has once commenced, the vessels may be displaced to a certain extent, and repelled even to the external angle of the crural opening; 4th, because it is the most dependent point of the iliac fossa, and uniformly that to which the action of the muscles has a constant tendency to propel the viscera. Thus, from anatomical inspection, we may conclude that external femoral hernia is a possible occurrence, but that it must be extremely rare; that the internal is the only one which has been observed; and that in this species, the protrusion almost always takes place at the fossette, which separates the umbilical ligament (artère) from the epigastric artery, because in the small number of cases in which it commences at the internal inguinal fossette, the organs protrude through the external opening of the inguinal canal.

Recapitulation. The crural ring is a broad opening, of greater extent transversely than from before backwards; broader internally, because its pubic angle is rounded off by Gimbernat's ligament, than towards its iliac angle, which is generally very acute, and includes the crural artery and vein. It is divided by these vessels into two portions; one, which is internal to the vein, triangular, blocked up by cellular tissue and a lymphatic gland, comprising half of the opening, and constitutes the crural ring, properly so called; the other external, triangular likewise, but principally occupied by the crural vessels, which leave on their iliac side only a small space, filled by a dense cellular tissue.

4th. *The Muscles.*

The iliac fossa contains the iliac and psoæ muscles.

(*a.*) The psoas parvus presents for consideration in this place, its tendon only. It should be observed that, on account of its relations with the fascia iliaca, some authors have con-

sidered this aponeurosis as a mere expansion of the tendon of this muscle; but, in order to prove that it is an accessory organ only, it is sufficient to state, that when the psoas parvus is wanting, this fibrous membrane still exists. Be this as it may, as this tendon descends it gradually expands, and is inserted, on the one hand, by means of the fascia iliaca, into the side of the margin of the pelvis; on the other, into the linea ilio-pectinea (crête ilio-pubienne); whence it follows, that its principal action is to stretch the aponeurosis of the iliac fossa, and indirectly the deep layer of the fascia lata. For the same reason, it may assist in flexing the pelvis on the spine, and should be relaxed when we attempt to reduce a hernia, or relieve its stricture when strangulated.

(*b.*) The psoas magnus forms almost the whole of the internal prominence mentioned in the commencement of this section. As it takes a direct course from the sacro-vertebral angle to the crural arch, it consequently diminishes the bis-iliac diameter of the superior strait of the pelvis, and hence the indication for slightly flexing the thighs and legs during parturition. Its external border receives almost all the fibres of the iliac muscle; so that these two muscles are confounded with each other in their passage under Poupart's ligament. The former being very thick superiorly, and the latter, on the contrary, very thin, they thereby limit a groove which gradually disappears in proportion as these two muscles become intermingled with each other. As the iliac muscle originates from the whole of the venter of the ilium, it is thin at its upper portion, but increases greatly in thickness previous to its passage under the crural arch.

In relation to these muscles, the iliac fascia forms the anterior half of a species of sac, open at its two extremities; that is to say, that it is inserted, externally, into the whole length of the spine of the ilium; internally, into the sides of the last lumbar vertebræ and brim of the pelvis, as far as the crista of the pubis* (crête pectinée); inferiorly, into the posterior margin of Poupart's ligament; and superiorly, it is pro-

---

* As the crista of the pubis is a name frequently given by English anatomists to the or tubercle spine (tuberculum ossis pubis), it is necessary to

longed upon the psoæ muscles, becoming more and more attenuated in proportion to its ascent;—so that this sac, which is very broad in the middle, becomes narrower below, and descends into the thigh through an opening formed, on the one hand, by the anterior border of the os innominatum, between its anterior and superior spinous process and the ilio-pectinated eminence* (éminence ilio-pectinée); on the other, by the external half of Poupart's ligament. This opening is elliptical, like that of the crural ring, to which it bears a resemblance in more than one respect, and from which it is separated by that portion of the fascia iliaca which descends from Poupart's ligament upon the body of the pubis. It also has two angles; an external, more elevated, and situated near to the anterior and superior spinous process of the ilium; and an internal, on a more inferior plane, falls upon the ilio-pubic eminence. It further resembles the crural sheath, in becoming narrower as it descends, forming a species of canal. It is completely filled by the psoas and iliacus muscles which pass through it to their termination at the trochanter minor, and also gives passage to the anterior crural nerve, and the inguino-cutaneous of the lumbar plexus. The superior opening, which is much more irregular, is filled by the psoæ, and is pervious only on the outer side of these muscles, because the layer of the fascia iliaca, which abandons the spine of the ilium in order to rise over the psoæ, is continued upon the anterior surface of these muscles throughout the whole of the lumbar region. Posteriorly, it is bounded by the ilio-lumbar ligament, the transverse process, and side of

---

state, that it here refers to that sharp ridge which forms the posterior and superior margin of the horizontal ramus of the pubis.—TRANSLATOR.

* The ilio-pectinated eminence is observed at the superior and anterior part of the os innominatum, above the cotyloid cavity. Its direction is oblique from without inwards and from behind forwards, and is formed by the junction of the base of the os ilium with that of the pubis. *Anatomie de Sabatier*, vol. i. p. 134. *Bichat Anatomie Descriptive*, vol. i. p. 190. *Portal Anatomie Médicale*, vol. i. p. 337. We will hereafter call it the "ilio-pubic eminence."—TRANSLATOR.

the body of the last lumbar vertebra. Finally, in order to penetrate into the iliac canal, without lacerating the fascia iliaca, it would be necessary to introduce the finger between the quadratus lumborum, the psoas, and the posterior curvature of the spine of the ilium.

We may now readily perceive that the muscles of the iliac fossa are firmly bound down in this species of canal, and that they can contract without communicating their movements to the digestive organs situated before them. We also see that the fibrous lamina which covers the psoas, unites the fascia iliaca to the ligamentum arcuatum, and, consequently, to the aponeurosis of the transversalis muscle; that a long fibro-osseous canal extends from the diaphragm to the trochanter minor, which is thin in its superior fibrous portion, where it is spread over the psoas muscle; depressed in the middle, where it is expanded over the iliac muscle; and terminates in a point without the pelvis, where we will again recur to it.

It is important to recollect this arrangement, inasmuch as it gives us a clew, by means of which we may be enabled to discriminate between the different forms of lumbar and psoas abscesses. Thus, it is well known that the pus, which results from vertebral caries, frequently descends, and forms an abscess in the groin, and that it then penetrates, either through the cellular tissue of the loins, or of the iliac fossa: but authors have omitted mentioning that it may descend along the fascia propria, between the peritoneum and the iliac fascia, when it must make its escape from the pelvis by the crural canal, rather than by passing through the canal which has just been described. In the first case, we include every infiltration and sub-peritoneal suppuration, as well as the products of affections of the vertebræ; but they rarely produce an abscess in the thigh, because the fluids are more disposed to make their way into the cellular tissue of the pelvis than to permeate the crural canal. In the second, to which appertain the consequences of psoïtis, of deep-seated caries of the vertebræ, not only of the lumbar, but likewise of the dorsal portion, the pus insinuates itself under the fascia iliaca through the superior aperture, and follows the iliac muscle, without being able to

make its escape otherwise than by the external opening of the crural arch, unless it should penetrate the different layers of the transversalis muscle above and on the outer side of the ilio-lumbar ligament, and make its appearance under the skin of the loins. For the same reason, when deep-seated abscesses of the loins disappear, in order to take their course to the groin, they pass into the iliac fossa under the fascia iliaca. We will have occasion to pursue the investigation of this subject when we come to treat of the inguinal region: but we may premise, that abscesses of the first species are more particularly consequent to inflammation of the cellular tissue, or of the soft parts in general, and are not necessarily connected with affections of the bones; whereas those of the second, which are more common, generally attend upon caries of the vertebræ.

5th. *The Arteries.*

The iliac arteries result from the bifurcation of the aorta, and consequently originate opposite to the inter-vertebral cartilage of the fourth and fifth lumbar vertebræ; sometimes a little above, at others a little below this point. Pestche, cited by F. Meckel, has seen them separate from the aorta very near the diaphragm. From their origin they diverge towards the body of the pubis, so that their direction may be pretty accurately represented by a line dropped from two inches below the umbilicus upon the middle of Poupart's ligament. As far as the posterior border of the body of the pubis, the length of the artery of the left side is four inches and a half, whilst that of the right is five inches: to the middle of Poupart's ligament it is half an inch more. This difference in length is owing to the situation of the aorta upon the left side of the spine. As the former abandons the sacro-vertebral angle, it makes a considerable curve, which extends a little beyond the sacro-iliac symphysis, the convexity of which is turned backwards and slightly outwards; afterwards it makes a longer but much gentler curvature, which is bent in an opposite direction, and extends to the middle of Poupart's ligament. After the iliac arteries have run to the

extent of two or three inches, they divide, or bifurcate in their turn: thus far they are called common or primitive iliacs, and the branches resulting from their division derive the names of internal or pelvic iliac, and external or crural iliac. The point at which this bifurcation occurs is not always the same: generally it is between the sacro-iliac symphysis and the sacro-vertebral protuberance; sometimes upon the symphysis itself, and in other cases upon the bodies of the vertebræ. It is seldom that this bifurcation does not take place, on the right side, somewhat nearer the spine, whilst on the left it is less remote from the sacro-iliac symphysis, which may be the case without the absolute length of the primitive iliacs being greater on the one side than on the other, because the artery of the right side is obliged to pass over a greater space before it reaches the sacro-iliac junction. It follows, however, from this disposition, that the external iliac arteries are of different lengths, the right being nearly an inch longer than the left. Although these varieties of origin may, at first sight, appear of but little consequence, they would nevertheless be in reality very important, if it was possible to ascertain them during life, when we wish to tie one of these arterial branches. It is indeed evident, if the disease requires the application of the ligature high up, that it will be attended with greater danger when the bifurcation exists low down, and *vice versa*. It is also evident from this circumstance, that the ligature of the right external iliac affords a greater probability of success than that of the left, but that the performance of the same operation upon the internal iliac (hypogastrique) artery would be attended with much greater difficulty. From the aorta as far as the crural arch, the iliac arteries are enveloped in a sheath of considerable strength, which they derive from the fascia propria; a sheath which is thicker before than behind, much stronger below than above, and in which the vein is also included. Externally, they lie upon the fascia iliaca, which separates them on the inner side from the psoæ muscles; posteriorly, we find the vein which is in contact with them, and the obturator nerve; anteriorly, they are covered by the peritoneum, crossed by the ureter opposite

to the sacro-iliac symphysis, and by the vas deferens near the crural arch. Frequently also the genito-crural nerve, or one of its branches, creeps along the anterior and outer surface of the artery included between the layers of its external tunic. This small cord deserves particular attention, in order that it may be avoided when tying the vessel, which is likewise surrounded by lymphatic vessels and glands.

The most important of all these relations, are those which exist between the artery and vein, between the artery and the sheath formed by the fascia propria, because in order that the former may be properly tied, it is necessary to avoid the second and divide the latter. But we will leave the further consideration of this part of our subject until after we have passed in review every thing relative to the artery.

The primitive iliac, previous to its bifurcation, sometimes gives off the ilio-lumbalis artery, which immediately glides between the sacro-iliac groove and the iliac and psoas magnus muscles, ramifies principally in the iliac fossa, where it anastomoses with the circumflex iliac, and, a little above, with the last lumbar branches; so that, in case the aorta was obliterated above its bifurcation, this artery would perform an important part.

Another much more fortunate disposition, if it existed in persons requiring the performance of this operation, would be that in which a renal artery originates from the iliac, an occurrence which we have sometimes met with.

The small capillary vessels which it afterwards distributes to the cellular tissue and lymphatic glands, are of no importance in surgery. The same may be said of those given off by the external iliac until within a few lines of its termination, where the epigastric and circumflex iliac arise, which we will now examine at their origin.

(*b.*) The epigastric artery generally originates from the anterior and internal part of the external iliac, on a level with the crista of the pubis. It seldom arises by a common trunk with the circumflex iliac, but it may come off at a lower point than the one just indicated: we have seen it originate from the femoral, and even the profunda. We sometimes find it

given off in the iliac fossa an inch behind the pubis. The first case would increase the danger of tying the crural artery in the upper part of the thigh between Poupart's ligament and the origin of the profunda; but it would be more favourable for the ligature of the external iliac. The second, on the contrary, might render the latter operation dangerous, and would favour the success of the former. Indeed, it is almost proved to a certainty that a ligature applied around a large artery, a little below the origin of a considerable branch, is liable to cut through the vessel before its obliteration is complete and solid, because this branch will turn off the current of the blood above, and thereby prevent the formation of the adhesive clot; so that it would be very dangerous, for example, to tie the external iliac artery between Poupart's ligament and the origin of the epigastric, which may readily happen when the latter branch is given off higher up than usual. This operation was performed by Béclard, in 1822, at the Hospital of La Faculté. On the fourth day the patient died from hæmorrhage, and it was ascertained that the ligature had cut through one half of the external iliac artery, two lines below the origin of the epigastric.

The epigastric artery, immediately after its origin, descends a little obliquely inwards, until it arrives opposite to Poupart's ligament, then ascends behind this ligament, describing a quarter of a circle around the inferior and internal portion of the spermatic cord.

We have already considered the relations of this vessel with the neck of the sac in inguinal herniæ, and will now investigate those which it contracts with crural herniæ. Thus, when the viscera pass into the crural ring, the external semi-circumference of the tumour, from behind forwards, is always embraced by the external iliac vein, the artery of the same name, and the origins of the epigastric vessels; so that, if we should attempt to remove the strangulation by dividing the stricture outwards and backwards, we might kill the patient by wounding the large arterial trunk; and if we attempt to dilate it upwards and outwards, it will be quite as difficult to avoid the epigastric artery. It is only towards the internal semi-circum-

ference of the tumour then, that the edge of the bistoury must be directed, in order to avoid hæmorrhage. But on this side also the epigastric artery distributes two branches, which merit our attention: one, which follows the posterior surface of Poupart's or Gimbernat's ligament, in order to cross the symphysis of the pubis; the other, which is frequently given off from this branch, crosses the posterior surface of the same ligament in its descent towards the obturator foramen. The origin of the first of these branches is sometimes a few lines lower, at others a little higher than usual; whence it follows, that in taking its horizontal course towards the pubis, it may run along the inferior border of Gimbernat's ligament, or pass parallel to, and even above, its superior margin;—so that, in crural herniæ, it will turn round the inferior part of the neck of the sac, in the first case; whilst, in the other, it will pass upon its anterior semi-circumference. It is the latter distribution which has alarmed surgeons, when dilating the stricture, and has induced them to cut inwards, instead of vertically, or parallel to the linea alba. But when this branch does not give off the obturator artery, it is seldom so large as to cause any apprehension from hæmorrhage. However, Mr. Hey, in his Surgical Observations, relates a case cited by Scarpa, in which he says that he wounded the epigastric artery in dilating the stricture upwards; and that the hæmorrhage was easily arrested by introducing small pieces of sponge into the wound. As the patient recovered, there was no positive proof of what had happened: but it appears evident, as Scarpa observes, that the epigastric was not the artery which was wounded, since it is almost uniformly external to the sac in crural herniæ. It is also very difficult to believe with the latter author, that it was the obturator originating from the epigastric or crural artery; but think that it was merely the branch which we are now describing, of greater dimensions than usual.* It is, in fact, very doubtful

---

* Vide Supplément au Traité sur les Hernies, de Scarpa, traduction de M. Olivier, p. 84.

whether compression alone would have prevented a return of the hæmorrhage, if it had been derived from either of the arteries indicated by these two authors.

The second (obturator) artery is as frequently given off from the branch which has just been described, as from the epigastric trunk itself; but it may originate near to the latter vessel; in which case, as it passes towards the obturator foramen, it will run over the external part of the neck of the sac in crural herniæ; whereas if its origin is more distant from the epigastric, it will turn over its internal surface. But if the branch from which it is derived passes below the tumour, which is fortunately most frequent, then there will be no important vessel on its internal and superior part; and as the spermatic artery in the female excites no apprehension, we may safely dilate the stricture in this direction. In the former case, on the contrary, the neck of the sac is surrounded by an almost complete arterial circle; so that, under the like circumstances, it would appear, at first sight, impossible to avoid a serious hæmorrhage in the operation for crural herniæ.

It is long since anatomists have noticed this common origin of the epigastric and obturator arteries; and Monro is the first who attempted to ascertain in what proportion these cases existed. This celebrated surgeon supposed that this anomaly occurs once out of twenty times. Burns met with it upwards of thirty times. Hesselback considers it a rare occurrence. Scarpa and Lawrence state the proportion as one in ten or fifteen; Cloquet, one out of five; and F. Meckel says that it is met with almost as frequently as the regular state. Our own observations lead us to concur with Monro. However this may be, it is easy to comprehend in what manner this anomaly in the origin of the obturator artery takes place. In the very young fœtus, this branch consists of two twigs, of nearly equal volume: one of these twigs is derived from the internal iliac (hypogastrique), the other from the external iliac, or the epigastric. Both will still exist after birth, and in the adult; but one of them will remain stationary with respect to its growth, whereas the other will enlarge in pro-

portion to the development of the organs to which it is distributed. If, therefore, contrary to the regular order, the branch from the internal iliac is stopt in its growth, the obturator will appear to originate from the epigastric.* We see then, that this arrangement is almost natural: it must only be observed, that in the greater proportion of these cases, when the obturator artery originates from the epigastric, it is driven outwards and backwards by the viscera which protrude in femoral herniæ, and that it will pass above and on the inner side of this tumour only when it arises from the pubic branch, properly so called.

In a case of double femoral herniæ existing on the same side, Burns found the obturator artery descending between the necks of the two tumours; so that the stricture of one of them might have been dilated inwards without danger, whereas the same direction of the incision in the other, would almost inevitably have been attended with hæmorrhage.

Even when the obturator artery turns over the superior surface of the hernia in passing towards its destination, it does not necessarily follow that it will inevitably be divided by cutting upon the concave margin of Gimbernat's ligament. On the contrary, its anatomical relations (with respect to the hernia), and observation, prove that it may be, most frequently, avoided. For in crural herniæ, this fibrous border is generally inclined towards the thigh—the vessels are more or less removed from the circumference of the crural ring by the viscera which traverse it, and they are naturally enveloped in the fascia propria, which retains them at a certain distance from the posterior surface of the external pillar of the ring, or the inguinal ligament of Gimbernat: so that, even under the most unfavourable circumstances, it will be almost always possible to cut upwards or inwards, to a sufficient extent, without wounding the arterial circle which circumscribes the neck of the hernia. The danger will also be less when the obturator arises from the epigastric near to its origin, or when

---

* Meckel, Manuel d'Anatomie, &c. tome 2, page 448.

the latter vessel is detached from the external iliac at a greater distance behind the crural arch: when, on the contrary, they originate together from the femoral artery itself, that is to say, between the crista of the pubis and the origin of the profunda, if the obturator did not pass between the tumour and the pectinalis muscle, if it ascended upon the anterior surface of the neck of the hernia, we conceive that, as it is much nearer to the seat of the stricture, it will be much more exposed to the action of the instrument.

There is also another anomaly, with which it is important to be acquainted: it is when the epigastric arises from the obturator, or some other branch of the internal iliac (hypogastrique). This disposition has been met with by Monro; and another very remarkable example of the kind is delineated in a late work of Hesselbach's. F. Meckel, who considers the latter author to be under a mistake in this respect, had doubtless not seen the beautiful plates in which this distribution is represented. When such a distribution exists, it will be the more detrimental, because the tumour will be free from arteries upon that side only at which we never dilate the stricture; for, of whatsoever species the hernia may then be, the epigastric artery will alike cross its internal surface. But we are induced to believe that, even in this case, the artery will be so far from the circumference of the crural ring, as not to be very much exposed to the action of the bistoury, if properly directed.

(*c.*) The circumflex iliac (iliaque anterieure) is also given off from the external iliac artery previous to its entrance into the crural canal: it generally arises opposite to the epigastric, i. e., from the anterior and external part of the crural artery. This origin is much more constant than that of the epigastric, and requires the same precautions when it becomes necessary to apply a ligature around the external iliac artery. Immediately after its origin it passes outwards, crosses the external iliac fossette, and ascends towards the anterior and superior spine of the ilium. When it arrives almost to the external angle of the great crural aperture, it perforates the fascia iliaca, which splits near its attachment to Poupart's

ligament, in order to form a sheath for the circumflex iliac artery: beyond the spinous process of the ilium, this vessel follows the curvature of the iliac spine, still enveloped by its fibrous sheath. From the concavity of this arterial arch a considerable number of branches originate, which inosculate with the ilio-lumbalis; whilst those derived from its convexity, ramify in the abdominal parietes. This artery is sometimes double, in which case the largest branch generally ascends almost perpendicularly into the parietes of the belly. We have already mentioned that this anomaly might render the operation for external inguinal hernia, and paracentesis abdominis, dangerous. As the circumflex iliac artery crosses the external iliac fossette, it is more approximated to the iliac fascia than the crural ligament;—so that, if the viscera should actually protrude at this part, the neck of the hernia would be surrounded, internally, by the external iliac and epigastric arteries; anteriorly, by the vas deferens; externally, by the spermatic artery; and posteriorly, by the circumflex iliac. To this we may also add, directly upon its anterior part, the entire spermatic cord which would be separated from the neck of the sac only by the thickness of Poupart's ligament, which is generally very thin in this situation.

But it is necessary, in this place, to give a cursory examination of the relations of the spermatic cord with the crural canal, especially as it respects the spermatic artery.

(*d.*) The spermatic artery originates from the aorta, or renal artery; descends in the fascia propria upon the anterior surface of the psoæ muscles, as far as the posterior surface of Poupart's ligament, without presenting much interest, in a surgical point of view; by degrees it approximates the vas deferens, ascends to the opening in the fascia transversalis, through which it passes into the inguinal canal. It is here that it becomes important in man, relative to crural hernia. Indeed, the spermatic cord takes such a direction that, when it enters the inguinal canal, it is situated at the iliac side of the crural hernia, whereas on making its exit from the external ring, it is on its pubic side; so that it crosses the whole of the superior part of the tumour, and by cutting upwards

the spermatic artery may be divided. Arnaud, who states that he lost a patient in consequence of this division, and relates the occurrence of a similar accident under another surgeon, was so convinced that the hæmorrhages previously referred to a wound of the epigastric, arose from the section of the spermatic artery, that he afterwards preferred raising the crural arch with a crotchet, and dividing the neck of the sac with the scissors, to making use of the bistoury. Sir A. Cooper, and Scarpa, have since participated in these apprehensions, and the opinion of these two celebrated surgeons is at present almost universally adopted. It is, for these reasons, that the former has advised making an incision through the aponeurosis of the external oblique, above and parallel to Poupart's ligament, and drawing the spermatic cord inwards and backwards (by means of a bent probe), afterwards passing a grooved director behind and below the crural ligament, in order that the bistoury may be more safely introduced upon it;—that the latter, in common with Gimbernat, Boyer, and most modern practitioners, prefers dividing the triangular expansion of the external pillar of the ring, by directing the edge of the instrument inwards and downwards towards the crest of the pubis, that is to say, parallel to the direction of the crural arch. The latter method of operating most certainly deserves the preference in man, and even in woman, inasmuch as there is no artery in this direction which it would be dangerous to wound; whereas externally, we will find the epigastric in both sexes; and anteriorly, we run the risk of dividing the spermatic cord or artery in the male, and the pubic branch in either sex. But we do not think that there is such an absolute necessity for the occurrence of these accidents, as Scarpa, A. Cooper, &c. seem to believe; and, most assuredly, the former has too greatly magnified the dangers which will follow from cutting in any other direction than that which he advises. Gunz certainly erred, when he affirmed, in opposition to Arnaud, that the spermatic artery could not be wounded, unless Poupart's ligament was completely divided; for, as we have seen, this ligament is hollowed out into a groove, and in the bottom of this groove

the artery takes its oblique course, in such a manner that it is situated a little nearer the neck of the hernia than would, at first sight, be imagined; but this groove also becomes insensibly obliterated, in proportion as the external pillar of the ring approximates the spine of the pubis, and the pillar itself gradually increases in width; so that, if we should cut from below upwards upon its inferior margin, near its internal extremity, we would not actually reach the spermatic cord and its vessels, unless we make a much more extensive incision than is necessary in the greater proportion of these cases. Sabatier recommends dividing the stricture in the direction of a line which would terminate at the umbilicus; and we do not find that he was very apprehensive of hæmorrhage, or of wounding the spermatic artery. Even externally, where the latter artery, and uniformly the epigastric trunk, are met with, observation has proved that the operation was rarely followed by hæmorrhage. Thus Sharp operated for crural herniæ, in both sexes, in the same manner as for inguinal herniæ—that is to say, he cut upwards and outwards towards the flank. We know that Mr. Dupuytren follows the same method; and the recent editors of Sabatier inform us that hæmorrhage is never the consequence of it. We must observe, however, that there is one essential peculiarity in Mr. Dupuytren's mode of operating, which consists in his using a bistoury with a convex cutting edge, or Pott's bistoury reversed, which permits him, he says, to overcome the stricture before the instrument has penetrated so far as the epigastric vessels, which are enveloped in the sub-peritoneal cellular tissue; and, on the other hand, it appears, from the obliquity of the incision, that the spermatic cord must be out of the way of injury. It is evident, according to this idea, that the straight bistoury of B. Bell might be used with greater facility and safety than that of Professor Dupuytren; and we think that the point of the common bistoury would answer the purpose still better. In all cases, it will be more prudent to carry it upon the base of Gimbernat's ligament than upwards and outwards, for it is clear, if the arteries are at all in danger, that we will find them in the latter direction; whereas

inwards, it is only where the obturator artery, originating from the epigastric, passes over the neck of the sac in its way to the obturator hole, that hæmorrhage will be liable to happen. We may further remark that the cord, loosely enveloped in the inguinal canal, will sooner glide from under the pressure of the instrument than be divided, and that the spermatic artery itself is, not unfrequently, of too small a calibre to give rise to a very serious hæmorrhage. We may conclude, therefore, that if Gunz appeared too confident of avoiding this artery, Arnaud and Scarpa, on the other hand, have greatly exaggerated the chances of wounding it.

(*e.*) In addition to the epigastric and circumflex ilii arteries, we sometimes, though very rarely, find a *Renal* branch originating from the external iliac. During the last winter we met with an anomaly of this nature on the right side; the kidney was very large; it received its emulgent from the aorta as usual, and the external iliac also sent to it another artery of almost equal volume. Such an arrangement would be very favourable if we were obliged to tie the aorta; but it might be very dangerous if it was required to put a ligature around the external iliac at a certain elevation.

In order that the external iliac artery may be tied with safety, it is absolutely necessary to be well acquainted with the arrangement of the organs which surround it at its entrance into the crural canal. We will, therefore, give it a moment's further consideration.

As the external iliac artery is passing upon the body of the pubis, it forms a considerable curvature, the convexity of which is directed forwards; whence it follows that this artery, behind the crural arch, lies deeper the nearer we approximate its origin; the peritoneum is reflected before it, from the iliac fossa upon the posterior surface of the fascia transversalis, forming a *cul-de-sac*, between which and the crural ring, there is a space which is filled by a lamellated and very extensible cellular tissue. At this point the application of a ligature will be less dangerous than elsewhere, but we must pay attention to the origins of the circumflex iliac and epigastric arteries, which should be left before, and the vas deferens which must

be pushed behind, if it encroaches too near the place where we wish to tie the vessel: the suppleness of the cellular tissue will admit a ready separation of the peritoneum, if it is necessary to carry the ligature a little further; but it must be recollected that the seminal duct, in descending from the abdominal orifice of the inguinal canal into the pelvis, crosses the artery which we are about to tie. The cellular sheath which envelopes the artery is very dense at this place, and appertains entirely to the *fascia propria:* it should be torn by the nail or grooved director, and the aneurismal needle passed upon the pubic side of the artery; then by moving it to and fro, the vein will be readily separated; whereas, if we introduce the needle at the iliac side of the artery, there will be great danger of perforating the vein. We must also be particular in excluding from the ligature the femoral branch of the genito-crural nerve, which almost always passes to the thigh through the external portion of the crural ring, and is sometimes enclosed in the laminæ of the sub-peritoneal tunic of the artery.

Among the methods adopted at the present day for tying the external iliac artery, there are three in particular which we ought to mention.

1st. That by means of which *Abernethy* first performed this operation, and which consists in making an incision parallel to the course of the artery above *Poupart's* ligament, cannot be generally adopted, as it is liable to wound the epigastric artery, the peritoneum and spermatic cord. In his subsequent operations he altered this method, that is to say, instead of directing the incision upwards and inwards, towards the umbilicus, he carried it upwards, and slightly outwards, and they have succeeded. In this manner the epigastric is more surely avoided, and the external iliac artery may be secured very high up, which must always be done when this vessel is diseased as far as the upper part of Poupart's ligament; but it is still somewhat difficult to avoid the peritoneum and spermatic cord.

2nd. That of *Sir A. Cooper*, who commences his incision within an inch of the anterior and superior spinous process of

the ilium, carries it in a similar direction, downwards to the upper part of Poupart's ligament, and again upwards, terminating it a little above the inner margin of the abdominal ring,—is attended with the serious inconvenience of making a wound which falls upon the artery by its inner extremity only: the epigastric artery, however, and the spermatic cord are completely avoided, and the peritoneum is very easily detached in the direction of this wound, since it is here connected to the ligament of *Fallopius*, by a great abundance of cellular tissue.

3d. That of *Bogros*, which consists in making an incision parallel to the crural arch, in such a manner that the middle of it will correspond to the external iliac artery, is but a modification of *Cooper's* method, and the latter only differs from the second operation of *Abernethy*, in as much as the external extremity of the wound is situated lower down and nearer to the spine of the ilium. As the incision of Bogros crosses the artery at a right angle, it presents the advantage of exposing it more certainly, but it requires greater address and precaution on the part of the surgeon, relative to the spermatic cord and epigastric artery; nevertheless, by combining it with that of *Cooper*, that is, by extending the incision to an inch and a half within the anterior and superior spinous process of the ilium, it seems to us to deserve the preference. The following will then be the order and arrangement of the organs which it will be necessary to divide or avoid: 1st. The skin, which must be cut to the extent of three inches, commencing the incision half an inch above *Poupart's* ligament, carrying it parallel to the direction of this ligament, and terminating it just above the superior angle of the external abdominal ring:—2nd, the subcutaneous layer enclosing the superficial epigastric artery and its branch, which must be secured as soon as divided; some veins of considerable size, and fat in greater or less abundance:—3d, the aponeurosis of the obliquus externus muscle, the fibres of which pass obliquely downwards and inwards to their insertion into *Poupart's* ligament, from which circumstance they cannot be separated without being cut across, except by following a method inter-

mediate to *Abernethy's* and *Cooper's*: 4th, the obliquus internus muscle, and the cellulo-adipose membrane which separates it from the preceding aponeurosis. It is necessary to use great caution in dividing these parts, and to dissect from the iliac towards the pubic portion of the wound, for the spermatic cord runs along the internal third part of the incision, and is in danger of being wounded. As this layer varies in thickness in different cases, we will consequently reach the peritoneum more speedily at one time than another, and as the division of it is one of the most dangerous steps in the operation, surgeons have advised laying aside the knife, and tearing with the finger or director, the soft muscular fibres of the internal oblique, as well as the cellular tissue which separates them anteriorily from the external aponeurosis, and posteriorly from the fascia transversalis. The divided parts, and the spermatic cord in particular, must, in all cases, be drawn upwards and inwards from the groove of *Poupart's* ligament. It must, nevertheless, be admitted, that until we have got thus far in the operation, there is no danger in using instruments, since two membranes yet remain to be divided before we reach the peritoneum, especially if we confine the use of the instrument to the external half of the wound, in order that we may the more certainly avoid the spermatic cord and the epigastric artery; these parts being softer, and more moveable, may be easily repelled, without laceration, by the finger or director: 5th, the *fascia transversalis*, which is so arranged that it sometimes requires considerable force in order to lacerate it transversely, because its fibres fall perpendicularly upon Poupart's ligament, and by employing the bistoury, the peritoneum may be perforated. We here give the preference, however, to the cutting instrument; for there is no doubt that the laceration produced by the finger, or director, will have a great tendency to produce suppuration, and prevent immediate reunion. Therefore, by dividing the different layers, which have just been enumerated, stratum by stratum, it will be very easy to cut the fascia transversalis, at least its external portion, parallel to *Pou-*

part's ligament, and near its insertion into the posterior margin of this ligament. Or, we might trace the cord to the internal orifice of the inguinal canal, and pass a grooved director through this opening, between the peritoneum and the fascia transversalis, whereby we will be able to cut the latter upon the conductor with greater safety. We have already observed that the public portion of this fascia is thicker than that which is situated on the iliac side of the cord; but it is never indispensable to divide it; if we wish to do so, however, in order to obtain more room, the cord should be pushed upwards and inwards, and it must be recollected, that the epigastric artery lies directly behind. This last division is one of the principal inconveniences attached to the operation of *Bogros* taken in its whole extent; but this distinguished anatomist thinks that it should not excite apprehension, on the contrary, he considers that the epigastric will serve as a guide to direct us to the external iliac:— 6th, the cellular membrane, which is always very lacerable, and readily permits the peritoneum, covered by the *fascia propria*, to be pushed towards the pelvis; unless these laminæ are agglutinated, in consequence of previous inflammation, as occurred in the operation performed by *Dr. Post*.* The lips of the wound being separated, and the peritoneum thus pushed back, the arterial pulsations are readily distinguished, and we will always find the external iliac artery behind Poupart's ligament, opposite to the inner margin of the posterior orifice of the inguinal canal. All that is then necessary in order to seize the artery, is to divide the *fascia propria* which envelopes it, either by tearing it with the nail, or cutting it with the point of the bistoury, in the same manner as we would make an opening into a hernial sack. At this step, the iliac vein, the spermatic vessels, and the vas deferens, which we separate inwards, demand the strictest attention. The ligature also, as we have already said, must be placed higher than the epigastric and circumflex iliac arteries; indeed, less inconveni-

---

* Amer. Med. and Phil. Journal, vol. iv. p. 403.

ence will result from approximating the internal iliac, than by keeping too near the crural arch, if the arterial tunics are not perfectly sound below. We should not do it, however, unless necessity requires it; for we would be obliged to detach the peritoneum to a considerable extent, which might lay the foundation for a very extensive inflammation of the lax cellular tissue of the fossa iliaca and pelvis; which constitutes one of the most serious inconveniences resulting from this operation.

As the operation of tying the external iliac artery has already been performed in Europe and America upwards of thirty times, and in the majority of cases with complete success, it is useless to resort to argument in order to prove its advantages: we will only observe, that the circulation will be afterwards carried on by means of the numerous anastomoses existing between the epigastrica superficialis, the circumflex iliac, epigastric, and intercostal arteries, the lumbar, ilio-lumbalis, and internal mammary arteries; between the circumflex and all the superior branches of the femoral, and the gluteal, ischiatic, internal pudic, and obturatrix artery, &c.

6th. *The Veins.*

The number of the veins existing in the iliac fossa equals that of the arteries; but they are much more voluminous.

(*a.*) The *primitive iliacs*, originating from the vena cava upon the right side of the fourth and fifth lumbar vertebræ, contract relations with their *arteriæ comites*, which are somewhat different on the right side from those on the left; and in order to have an idea of this difference, as to their length and intimate relations, it is sufficient to transfer, in imagination, what we have said of the artery of the right side, to the vein of the left, and *vice versa*.* It will then follow, that the left

---

* A literal translation of the text, at this place, would read thus:—viz. * * * "it is sufficient to transfer, in thought, what we have said of the artery of the right side, to the vein of the left, and (*reciproquement*) of the artery of the left side to the vein of the right. Hence it fol-

iliac vein is obliged to cross the posterior surface of the commencement of the right iliac artery, and even of the aortic trunk, in order to place itself on the inner side of the left external iliac artery, so that the external surface of the latter is not in contact with the vein throughout its whole extent. The right iliac vein, on the contrary, is at first situated on the outer side of the right iliac artery, but passes gradually under its posterior surface, so that before it reaches the crural ring, it is situated on the inner side of the external iliac artery. It is important to recollect this arrangament in a surgical point of view. For we may readily see that, if we wish to pass a ligature around the inferior third of the external iliac artery, we will readily succeed by sliding the blunt extremity of the needle under the pubic side of the artery obliquely outwards, and the same method would be equally applicable to the whole length of the left external iliac artery, and even to the common trunk of this side; but on the right it would be necessary to act differently if we wish to avoid dangerous consequences; for at two inches and more behind the crural arch, it would be very difficult to avoid perforating the vein in attempting to pass the needle under the artery from within outwards, whilst in the opposite direction it would be much easier to separate the vessels, because, on the side of the pelvis, the instrument would meet with no resistance. Finally, the veins are enveloped in the same sheath with the arteries, and otherwise present the same general relations.

---

lows, that the latter (*right*) vein is obliged to cross the posterior surface of the commencement of the right iliac artery, and also of the aortic trunk, in order to place itself on the inner side of the right external iliac (*pelvi-crurale*) artery, so that the external surface of the latter is free throughout its whole extent. The left vein, on the contrary, is at first situated on the outer side of the artery, gradually slides under its posterior surface, and at length places itself on the inner side of the artery before it reaches the crural ring." Which is evidently a misprint, as it does not accord either with the anatomical relations of the parts, or the surgical observations which follow. In the translation, therefore, the words *right* and *left* have been occasionally transposed.

(*b.*) The *Ilio-lumbalis* and *circumflex ilii* veins, accompany the arterial branches of the same names: the two veins which accompany each epigastric artery are distributed in like manner, except that they do not form a curvature under the spermatic cord, because they descend directly upon the external iliac veins which correspond to the centre of the internal abdominal ring. It is evident, therefore, in consequence of this disposition, that if we wish to tie the artery, we will be able to separate it more readily from its accompanying veins the nearer we approximate its origin.

(*c.*) The *spermatic veins* also accompany the arteries, which they considerably exceed in volume; there are always two, and often three, particularly in the female, in whom these vessels are called ovarian, and they sometimes form a complete plexus, which extends from the kidnies to the broad ligaments, especially after repeated pregnancies. In this sex, and sometimes in man, the lumbar veins inosculate with branches of the circumflex and ilio-lumbalis, and frequently form, in and above the iliac fossa, another plexus called "*pampiniforme*," which must not be confounded with the preceding. The length of the ovarian and spermatic veins, the tenuity of their coats, and the softness of the textures through which they pass, are causes which frequently predispose to their varicose enlargement.

We have already said that the ureter crosses the iliac artery opposite to the sacro-iliac symphysis, and that the vas deferens passes over the anterior surface of the external iliac artery, in its descent from the inguinal canal into the pelvis. As these organs present nothing remarkable here, excepting their position, we will not occupy any more time respecting them.

7th. *The Lymphatics.*

This system is very abundant in the iliac fossa; the vessels are thick, short, and numerous; their glands form a species of chaplet around the blood-vessels, and both are enveloped in the *fascia propria*. Several of these glands lie directly upon the artery, and as they sometimes become gradually en-

larged from various diseases, such enlargement might be mistaken for aneurism, a mistake which may be easily made, both on account of the pulsations of the artery beneath being communicated to them, and the depth of their situation rendering their investigation difficult. One of these glands is almost uniformly situated in the crural canal, enveloped in a process of the fascia propria; and as this gland is generally very much elongated, it may project into the pelvis at the same time that it extends to the femoral opening of the canal, which it sometimes fills almost completely. The position of this organ renders it important, for should it undergo tumefaction, as it occupies a very confined space, it may give rise to serious consequences: thus, for example, it may compress the femoral vein or artery, and by impeding the reflux of blood through the former, occasion œdema of the limb; or, by interrupting the arterial circulation, give rise to numbness and more or less acute pains. It may likewise form a tumour in the groin, which may be mistaken for a hernia.

8*th*. *The Nerves*.

They are derived from the lumbar plexus; are at first imbedded in the fibres of the psoas muscle, and afterwards run under the aponeurosis to a greater or less distance previous to perforating this membrane.

(*a*.) The *Inguino-cutaneous* nerve runs obliquely between the iliac fascia and muscle, near to the anterior and superior spinous process of the os ilii, passes through the notch which separates this process from the anterior and inferior spine, and does not perforate the fibrous membrane until it reaches the thigh. Its deep situation prevents its compression, during obstinate constipations, by indurated fæces lodged in the cœcum, or sigmoid flexure of the colon.

(*b*.) The genito-crural nerve rests upon the anterior surface of the psoæ muscles in the sub-peritoneal cellular layer, and runs parallel to the external iliac artery, generally on its external side, occasionally upon its anterior surface; so that it may sometimes be very easily avoided when tying this vessel, whilst at others, it is so imbedded in the layers which cover it,

that it would be dangerous to attempt separating it. Under the latter circumstance, it would be better to seize it with the forceps and divide it, than include it in the ligature. If, however, it is already compressed by the noose, we should draw it tighter, in order that its function may be completely annihilated. Its two branches do not generally separate until they arrive near *Poupart's* ligament; the inguinal branch soon joins the spermatic vessels and vas deferens, and enters with them into the inguinal canal; the femoral branch follows the primary direction of the nerve, and enters into the external portion of the crural canal: but neither of them are so large as to be of any great importance in surgical diseases.

(*c.*) The *obturator* nerve descends perpendicularly under the psoas, and pierces the aponeurosis, which conceals the sacro-iliac symphysis, in order to pass across the pelvis, where it is situated behind the iliac artery and veins. We will see it hereafter in the pelvic excavation.

(*d.*) So with respect to the *sacro-lumbalis* nerve, which unites the two plexuses from which it derives its name, as it crosses before the ala of the sacrum.

(*e.*) The *crural (anterior crural)* is the principal nerve in this fossa. As it descends it gradually removes from the posterior surface of the psoas muscle, so that towards the inferior third of the iliac fossa, it is observed beneath the fascia iliaca, in the groove between the psoas and iliacus muscles: thus far it is separated from the artery by the muscle under which it passes, but afterwards approximates so close to it, that at its entrance into the crural canal, the aponeurosis alone is situated between them: this membrane, however, is so very thick at this place, that in all operations performed upon the artery, the nerve may be easily avoided. We may further state that this nerve is enclosed in the same fibrous sheath with the psoas and iliac muscles, and passes with them under the crural arch, filling the internal angle of the iliac canal; and that the fibrous partition, which separates it from the artery, or crural canal, is prolonged into the thigh, forming what Mr. *J. Cloquet* calls the "*septum crurale.*"

9*th.* *The Cellular Tissue.*

The cellular tissue of the iliac fossa is lamellated, abundant, and very supple, encloses but few adipose cells, and forms two very distinct membranes; one, which has been already described, is situated between the peritoneum and the *fascia iliaca*, passes through the inguinal and crural canals, and expands into the *fascia superficialis* of the thigh: the other, deeper, is separated from the former by the iliac fascia, surrounds the muscles and the nervous trunk, is continuous with the deep seated, or intermuscular cellular tissue of the thigh, by means of the iliac canal, and serves as a filter for the pus which accumulates in the greater proportion of psoas and lumbar abscesses. (*dépôts par congestion*)

10*th.* **The Skeleton.* (*le Squelette.*)

This consists of the osseous portion from which the name of the region is derived, and consequently comprehends the os ilii alone, which will be more particularly examined when we come to treat of the gluteal region. At present, we will only observe that the two iliac fossæ constitute almost the whole of what is called the *pelvis major*, which is much broader in the female than the male, a difference of breadth, which is especially dependent upon the sexual functions. The enlarged dimensions of the pelvis in the female, cannot exist without a very extensive separation of the iliac spines from each other, which consequently produces a greater elongation of the crural arch; and as the inguinal canal, in this sex is very small, it follows that crural herniæ are as frequent in women, as inguinal herniæ are in men. A proof that such

---

* The term *squelette* (skeleton) is made use of by Mr. Velpéau, to signify the frame-work of each particular region; and, therefore, comprises in several regions, not only the bones, but also the periosteum, ligaments, cartilages, &c. As no other word equally comprehensive and appropriate could have been substituted, it has been retained in the translation, notwithstanding English and American anatomists limit its application to the frame-work of the whole system. Tr.

is the cause of this difference in the frequency of crural herniæ is, that in childhood, when the dimensions of the pelvis are similar in both sexes, inguinal herniæ are almost as common in young girls as crural. It sometimes happens that the iliac spines are inclined backwards in such a manner, that the whole of the ilium approximates the horizontal direction; or, in short, that the iliac fossa becomes level, as *Mr. Ribes* has twice observed, in very aged females. Such a disposition would, in a special manner, favour the obliquity of the uterus during pregnancy, if it occurred in women young enough to become pregnant. It appears to us, that this is owing to the constant, but gentle pressure, which the viscera exercise upon the bones, when feebly supported by the relaxed abdominal parietes, whether this relaxation is owing to the advance of age simply, to repeated pregnancies, or emaciation subsequent to polysarcia, or ordinary *embonpoint*, as occurs in many women towards the decline of life. When, on the contrary, the spines of the ilia are more elevated, i. e. when they have the same position as in man, they will favour the descent of the womb during gestation, unless the pelvic cavity, on the other hand, is diminished. In a well-proportioned woman, the distance between each anterior and superior spinous process of the ilia, should be nine inches and a half, and ten inches and a half between the centre of each iliac spine. But we will resume this subject after we have examined the different regions of the pelvis.

The osseous fossa is so loosely connected to the iliacus muscle, that it is easily denuded by suppuration, which accounts for the extreme facility with which necrosis, or disorganization of the os ilii takes place, after deep-seated inflammation of the cellular tissue, *psoïtis*, &c.; and hence the necessity for attacking these diseases vigorously in their commencement, if we wish to prevent the death of the patients.

ART. III. *An Account of some* EXPERIMENTS *on the root of the* SANGUINARIA CANADENSIS. By the late JAMES FREEMAN DANA, M. D. Professor of Chemistry in the University of the State of New-York.

§ 1.

IN July 1824, I commenced a series of experiments on the blood-root, with a view to discover some method of separating the coloring principle, and of making it useful in the arts. The object of the experiments was not attained; but in the course of the investigation, some circumstances occurred which indicated the presence of alkaline matter, and the researches which have been made on it, have led to the inference that it is a peculiar vegeto-alkali. Whether it be entitled to rank or not, it will, in the following account, be called *Sanguinarine*, or *Sanguinarina*.*

§ 2.

The root, by drying, loses about one-seventh part of its weight. When the recent root is boiled in water, and the liquid filtered, a brownish yellow solution is obtained, which presents the following results by the action of re-agents:

1. Solution of pure potash produces a greenish precipitate, which, on the addition of acids, becomes of a fine red color.

2. Solution of chloride of gold produces a brown-colored precipitate, in which the gold is reduced to the metallic state.

3. Solution of deuto-chloride of mercury affords two distinct precipitates; one of a brick-red color, the other of a dark brownish-red hue.

4. Solution of proto-chloride of tin, produces a light flesh-red precipitate;

5. Solution of per-chloride of iron, a brown precipitate;

---

* In April 1826, a friend showed me a notice in the New-York Medical and Physical Journal, of a chemical examination of Blood-root by Dr. BIRD, who made it the subject of an Inaugural Dissertation. Alkaline matter was found in it, and *supposed to be the same* as that afforded by Peruvian Bark. I have never seen the original memoir.

6. Solution of binacetate of lead, a light clay-colored precipitate;

7. Solution of nitrate of silver, a dark brown precipitate;

8. Solution of per-sulphate of iron, a reddish brown precipitate; and,

9. Tincture of iodine produces a precipitate; but

10. The watery solution of iodine produces none, but a change only of color.

11. The infusion of the root in cold water, gave the same results with the above-named tests, and afforded also a precipitate with solution of silicated potash.

12. Water distilled from the recent root, has a slight empyreumatic odor, and nauseous taste.

13. A very dilute solution of carbonate of potash boiled on the root, acquired a fine tint of reddish brown: an acid dropped into it, caused the whole to become gelatinous.

14. Alcohol digested on the recent root, acquires a slight color: when this solution is poured into *cold* water, a milky fluid is the result.

15. Sulphuric ether digested on the dry powdered root, acquires, after a few hours, a yellowish color, and leaves a reddish brown matter undissolved. The etherial solution, when evaporated, leaves a resinous matter of a brown red color, slightly adhesive, and of a very acrid taste.

16. Pure dilute acetic acid digested on the root, acquires a bright red color, having an acrid taste like that of the root. The acetic solution, evaporated to dryness, leaves a red-colored mass, slightly adhesive, and of a very acrid taste. This red matter is soluble in water; and this watery solution affords a bright red precipitate, with proto-chloride of tin; is not altered by solution of alum, but is changed to a yellowish brown color by carbonate of potash. Some unsuccessful attempts were made to fix this color on cotton.

§ 3.

An analysis of the root of the S. canadensis, with a view to separate its proximate principles, was attempted, but the results were not satisfactory. A detail of the experiments is here stated, rather to show the utter insufficiency of some of

the usual modes of vegetable analysis, than for any other purpose.

(1.) Two hundred grains of the dried root in fine powder, were rubbed to a paste with cold water, in a clean glass mortar, and the whole transferred to a glass jar; cold water was then added, and the whole suffered to stand, with occasional stirring, for twenty-four hours. A clear yellow solution was thus obtained, which was cautiously decanted, and the remaining portion was repeatedly washed with cold water, as long as it appeared to extract any soluble matter. The solution thus procured was then concentrated by evaporation, and during this process its color became darker, and changed to a brown, and small insoluble flakes appeared. The whole was now evaporated to dryness at 212° F., and weighed 26 grains. The color of the substance thus procured, was not altered by acids, nor did it affect litmus paper. After standing a few days, it was found to have deliquiated; and on the addition of cold water, a partial solution was effected, which had a brown color, and produced on turmeric paper the usual effect of alkalies. A careful examination of this fluid showed that it contained carbonate of soda, nitrate of potash, sulphate of lime, and a trace of some compound containing chlorine.

(2.) The reddish brown residuum left after the action of cold water, was then digested in dilute alcohol, as long as that fluid acquired any color from it. The clear solution thus obtained, was of a beautiful red color; the alcohol was distilled off, the liquid became turbid, and when evaporated afforded a residuum weighing 2.1 grains. It contained resin, a trace of starch, by solution of iodine, and extractive matter probably altered.

(3.) The insoluble residuum of process (2.) was boiled in a large quantity of water; the insoluble portion separated by filtration, and well washed: it consisted of ligneous fibre principally, and when dried at 212°, weighed 102 grains.

(4.) The fluid (3.) was concentrated, and became darker colored: evaporated to dryness, a brown matter was obtained, which presented the characteristics of starch. Dried at 212°, it weighed 75 grains.

(5.) Two hundred grains of the dried root, when consumed in a crucible, left five grains of earthy and saline matter; in which carbonate of potash, carbonate of soda, carbonate and sulphate of lime, were detected: indications also of magnesia were observed.

The results of this analysis then are:

Matters soluble in cold water, (gum, extractive and saline)	*grs.*	26.
Matters soluble in dilute alcohol, (resino-extractive)		2.1
Matters soluble in boiling water, (starch, &c.)		75.
Insoluble residue, ...		100.
		203.1

Or,	Vegetable matter, . . .	195	*grs.*
	Saline and earthy matter, .	5	"
		200	

§ 4.

The analysis of vegetable substances, made in the manner above-mentioned, can be of very limited utility, except perhaps for the purposes of pharmacy. This analysis has failed altogether in separating the coloring principle, and the principle in which resides the acrid quality of the root; and the substances which it has presented to us, may perhaps with propriety be considered as products of the processes, formed by the re-action of the vegetable principles upon each other. Other experiments were therefore instituted, both with a view of obtaining the coloring and the acrid principle.

A portion of the dried root in fine powder was digested in absolute alcohol: the liquid, after the lapse of several hours, was separated by filtration; it was of a deep red color. A few drops of this solution evaporated to dryness on a watch-glass, afforded a light brown matter; water digested on it a yellow color, and an acrid taste; and the portion insoluble in water, became of a lighter color by the action of that liquid, and changed the color of turmeric paper to a reddish brown. It appeared probable that this effect was produced by a vegeto-alkali. A few drops of the aqueous solution of ammonia were added to a portion of the solution, which threw down

a gray precipitate: this was allowed to subside, and the fluid was decanted: water was then added, and boiled, with some freshly prepared and finely pulverized charcoal, and the whole filtered, and the portion on the filter was washed with cold water. Alcohol was then digested on the matter remaining on the filter, and afterward was evaporated to dryness: *a white pearly substance remained, having an acrid taste, and rendering brown the yellow of the turmeric, and changing the infusions, and paper stained with the infusion of the purple cabbage, to a green.*

§ 5.

To the substance thus obtained, I have ventured to give the name Sanguinarina, or Sanguinarine; but the first name is preferable, as the termination in *a* is in accordance with the name of the other alkaline bodies.

Sanguinarina may be obtained from the roots of the S. canadensis by several processes:

1st. By infusing the powdered root in warm water, and adding to it a small quantity of ammonia, potash, lime, or magnesia, a precipitate falls, consisting of the impure sanguinarina, which may be purified in the manner to be mentioned below.

2nd. By infusing the powdered root in water acidulated with muriatic, or acetic acid, (and probably other acids would produce the same effect,) a very red-colored solution is procured, from which ammonia, potash, lime or magnesia separate the impure sanguinarina. When thus procured, it is of a dark color, but may be purified by dissolving it in pure alcohol, and pouring the solution into cold water; a white precipitate instantly appears, which after some time subsides: or the alkali may be obtained pure, by boiling the impure substance with recently prepared charcoal, suffering the liquor to cool, and filtering. Alcohol poured on the filter, will dissolve the sanguinarina.

Sanguinarina has the following properties: When first obtained, it is white and colorless, but becomes of a yellowish white, or nearly buff-colored, when exposed for a long time to the air—a change which is probably due to the action of

carbonic acid. It is in the form of fine grains; its taste is extremely acrid; but it requires a long time to develope its taste, in consequence of its little solubility. The impression gradually extends over the mouth and fauces, and down the œsophagus, and becomes painful: sickness and debility ensue.

Sanguinarina is very sparingly soluble in water; but is soluble in ether, and very soluble in alcohol. It affords a precipitate with tincture of galls, insoluble in ammonia, but soluble in alcohol.

The alcoholic solution evaporated, leaves a white pearly crystalline crust or mass.

Sanguinarina combines with acids, and forms salts: *all the salts are colored, and present some shade of red, crimson, or scarlet, and the color is of great intensity and beauty.* The change of color which takes place when the white alkali is combined with a white and colorless acid, is a peculiarly striking effect. The muriate, sulphate, nitrate, phosphate, acetate, and tartrate, have been made the subjects of experiment: they all have more or less of an acrid taste: the muriate and the acetate are peculiarly pungent and acrid. The salts are soluble in water and in alcohol, and form red-colored solutions of great beauty. The white alkali, when exposed to the vapor of acids, instantly changes to a red.

When ammonia, potash, lime or magnesia, is added to the solutions of the combinations of sanguinarina with acids, the vegeto-alkaline matter is separated, or its salts are decomposed, and the sanguinarina is obtained in an unaltered state, if it has been acted on by a dilute acid; but if it have been combined with a concentrated acid, ammonia or the other alkalies will separate it in an *altered* state. Thus if the vegeto-alkali be added to strong muriatic acid, or strong nitric acid, a red-colored compound is procured, from which ammonia separates a dark purple precipitate. This precipitate is soluble in alcohol, and forms a reddish-colored solution, and appears to be a compound of the alkali and acid, in proportions different from those existing in the soluble compounds.

The coloring matter in the root of the S. canadensis, is a

compound of the sanguinarina and an acid, or a salt; and it is not known with certainty what acid exists in it, whether it be a peculiar acid, or one which is already well known. Mr. Augustus A. Hayes, an indefatigable and successful cultivator of science,—who was formerly my pupil, and subsequently assistant in my Lectures, and to whom I am indebted for much assistance in the examination of this vegetable,—is favorably situated for ascertaining the nature of the acid contained in the blood-root, and is engaged in prosecuting the inquiry. It is not improbable that the coloring matter of other vegetables may be found to consist of a salifiable basis and an acid: but I forbear to enter into the speculations which might arise from the facts now presented to the Lyceum. I believe that it may be considered established—

1. That the S. canadensis contains a peculiar vegeto-alkali, or salifiable basis.

2. That this vegeto-alkali possesses the general properties of similar bodies, but is distinguished from all others by forming *colored salts* with acids: and,

3. That the coloring matter of the blood-root is a vegetable salt.

An early opportunity will be embraced to present a more particular account of the combinations of this substance.*

ART IV. *Contributions in Medical Jurisprudence and Police.* No. IV.

(From a Correspondent.)

DURATION OF HUMAN PREGNANCY.†

ANOTHER case, "bearing the impress of vice," has again brought this subject before a legal tribunal. The character

---

* Reprinted from No. 8. Vol. II. of the Annals of the Lyceum of Natural History of New-York.

† The Medical Evidence relative to the Duration of Human Pregnancy, as given in the Gardner Peerage Case, before the Committee for Privileges of the House of Lords, in 1825—6. With Introductory Remarks and Notes, by Robert Lyall, M. D. F.L.S. London, 1826.

of the judges, the reputation of the medical witnesses, the importance of the decision to the contending parties, no less than the great interest of the physiological question, all unite in attracting attention towards it. We confess, however, that we have been exceedingly disappointed, not at the result, (which was unquestionably right) but with the evidence adduced. Although the case has been briefly mentioned in some of the English periodicals, yet, we believe, an abstract of it may not prove uninteresting to most of the readers of this Journal.

The Honourable Allen Hyde (afterwards Lord) Gardiner, a captain in the British Navy, was married to Miss Adderly, at Fort St. George in the East Indies, in 1796. On the 8th of December, 1802, Mrs. Gardiner bore a child, which appeared to be the fruit of an illicit intercourse between her and Henry Jadis, Esq. An action for criminal conversation was instituted by Lord Gardiner against Mr. Jadis, in the court of King's Bench, and he obtained a verdict of one thousand pounds damages. He then procured a sentence of divorce, in the Consistory Court of the Bishop of London, to which the royal assent was obtained on the 10th of July, 1805. Mr. Jadis married the late Mrs. Gardiner in 1805, and the child just alluded to, was acknowledged from its birth as their offspring, and took their name, viz. Henry Fenton Jadis, which he bore until the commencement of this suit, when he assumed that of Henry Fenton Gardiner. Lord Gardiner married a second time, with the Honourable Miss Smith, daughter of Lord Carrington, on the 10th of April, 1809. And a son, Allan Legge Gardiner, and the claimant of the peerage and estate in this case, was born to them on the 29th of January, 1810. Finally, Lord Gardiner died in London, January 5, 1816.

"In 1805, Lord Gardiner, who was then Captain of H. M. ship, the *Resolution*, arrived off Portsmouth, and was joined by his first wife, who remained with him on board about three weeks, and then took her departure for London, on the 30th of January. It appears, however, that the Resolution did not sail till the 7th of February, and that some communications

took place between the ship and the shore by means of boats. Lord Gardiner sailed for the West Indies, and returned home on the 11th of July of the same year."

These facts led to the consideration of the following questions.

1. Could a child, born on the 8th of December, have been the result of sexual intercourse, either on the 30th of January, or anterior to it, being 311 days?

2. Could a child, born on the 8th of December, have been the result of sexual intercourse on the 7th of February, or anterior to it, being 304 days.

3. Could a child, born on the 8th of December, and that has lived to manhood, be the result of sexual intercourse on or after the 11th of July, a period short, at least by two or three days, of five calender months?

In other words, this case involved a notice of two important subjects in medical jurisprudence.

1st. The earliest period at which a premature fœtus or infant is capable of living to maturity or manhood. 2nd. The longest period to which utero-gestation can be protracted.

The former does not appear to have been much discussed; the medical testimony being confined to the other and more probable explanation, if the case was susceptible of that explanation.

Seventeen medical gentlemen, and some of them the most distinguished accoucheurs now in London, were examined as witnesses, besides several females of great experience (as will be seen) in child bearing. Their opinions may be noticed with reference to their belief, or disbelief, in protracted utero-gestation.

Drs. Gooch, Ralph Blegborough, and David Davis, and Messrs. Charles M. Clarke, and R. P. Pennington, may be considered as not crediting it.

Dr. Gooch stated that he had been in considerable practice for upwards of sixteen years; that in his opinion, the usual period of gestation, where it could be calculated accurately, was *nine calendar months*, as from the 25th May to the 25th of December.

The statement commonly given in books, that forty weeks is the usual term, he conceived exceeded the true duration. The writers say, "nine calender months, or forty weeks;" now the fact is, that nine calender months is "scarcely more than thirty-nine weeks." The only latitude allowed by this witness was, that the period might be a day or two more or less. He formed this opinion, from meeting with instances where the time of conception is accurately known. The statement of Dr. Hunter was urged to him, that he (Dr. William Hunter) "had *known* a woman bear a living child, in a perfectly natural way, fourteen days later than nine calendar months; and *believed* two women to have been delivered of a child alive in a natural way, above ten calendar months from the time of conception." Dr. G. professed the highest respect for the character and talents of Dr. Hunter, but he entertained doubts as to the accuracy of these cases; he should like to know the grounds on which the opinions were formed; and how far they depended on the testimony of the female individuals. On being asked if such a case should fall within his knowledge, how he would account for it, he replied; "on no other ground, but the circumstances deviating greatly in *some rare instances* from the ordinary course of nature: but I beg leave to add, that I have seen none similar to it in the course of my experience."

On his cross examination, inquiry was particularly made as to those cases in which he supposed an *accurate* calculation could be made. He stated, that he was for many years physician to two lying-in hospitals. In one of these, there are two wards kept for single women; "so that cases frequently occurred, in which I had an opportunity of calculating accurately the length of pregnancy." Young females, he added, in very respectable situations are often seduced; *the intercourse is single, and there is no motive whatever for mistaking the fact.* It would appear that Dr. Gooch relied much for his opinion on such cases, and did not believe that the obvious objection to such testimony, (viz. that the confession of more numerous connexions would give a suspicion of general incontinence) would lie in the instances he had seen.

Dr. Ralph Blegborough has been in practice in London thirty-four years. He considers thirty-nine weeks as the period of gestation, but forty as the ultimatum. Mechanical obstructions, as from malconformation, might delay birth for five or six days; but in that case it is uniformly attended with hazard, and almost certain death, either to the mother or the child, or to both. This witness mentions the following circumstances, on which he grounded *his calculations*. "It is not unusual to be sent to by ladies who have felt a peculiar sensation; have fainted, and have been extremely ill, so as to induce their friends to send for a professional man; upon examining them minutely, and asking them those questions which are proper on the occasion, they will declare certain sensations, by which we know that conception had taken place, and was the cause of those feelings which they represented to us. Upon calculating from that time, I have in such instances, invariably found I have been right in my surmises, and that labour has taken place certainly not later, in every instance that I recollect of this sort, than *forty weeks* from that period."

On cross examination, Dr. B. admitted that these symptoms do not necessarily follow immediately upon the sexual intercourse, but that they do so *frequently*.

Dr. Davis has practised midwifery thirteen years. The period of gestation is nine calendar months, and he should incline to a day shorter, or two days shorter, rather than beyond that period. He had formed this opinion from his own medical experience. He had met with a few cases, where the patients have reckoned (as their expression is) from a single coitus; and in all these cases, without a single exception, the children came on the thirty-ninth week; "at the conclusion of that week, I cannot say exactly on what day, and in all the others rather before that period, that is to say, within the thirty-ninth week."

Dr. Davis also observes, that when females reckon on general principles, as from menstruation, it sometimes happens that they are wrong.

Mr. Charles Mansfield Clarke had been in practice twenty years. He considers forty weeks as the full period of a female's gestation, under ordinary circumstances. In answer to various questions, he replied, that he never knew a case, in which premature fatigue and exhaustion had caused protracted gestation. He could understand that they may accelerate, but could not see how they could retard. He had never known a case retarded beyond the forty weeks; although he allowed, that labour might be protracted during several days. In *several* instances, the fact of the last intercourse had been stated to him by the parties, who alone were acquainted with it, for their mutual advantage; and he had combined that fact with the knowledge of the subsequent fact of the commencement of labour, and in no case had he found the forty weeks exceeded. He subsequently mentioned, that the number of instances of this kind which had come under his own observation, was at least twenty.

A question was asked of Mr. Clarke, as to the mode of calculation from the suppression of the menses. He deemed that he should take the safest mode of giving the average reckoning, by counting the half of twenty-eight days, fourteen from the last period, and of course fourteen from the next expected one; and he should offer an opinion, that in all probability, the effect might follow the cause at the end of forty weeks from that half of the interval; "but I should know that it must take place at the end of forty weeks from the day upon which the last menstruation ceased, and before the day of expected mens ruation." [This last remark is rather ambiguous, but the explanation given by Dr. Lyall, is probably what was meant—*within forty weeks of the day on which menstruation should have taken place, but for impregnation.*] Mr. Clarke never knew a case in which labour was protracted beyond forty weeks from the day when the next menstruation ought to have taken place.

R. R. Pennington had been an accoucheur thirty-seven years; had never known gestation protracted beyond three or four days after forty weeks; and forty weeks is the usual term. He formed his opinion from the time of conception,

and this was ascertained from circumstances stated by the females.

On his cross examination, he mentioned that some children were born in thirty-nine, and some in thirty-seven weeks.

This summary of the testimony for the claimant, will be best concluded by a tabular sketch of the periods allotted by each to gestation, and the circumstances on which they founded their opinions.

Dr. Gooch.	Nine calendar months, or thirty-nine weeks; or, from 271 to 277 days.	A single coitus.
Dr. Blegborough.	Thirty-nine to forty weeks, or from 273 to 280 days.	A peculiar sensation felt by females.
Dr. Davis.	Nine calendar months, or thirty-nine weeks; or, from 271 to 273 days.	A single coitus.
C. M. Clarke.	Forty weeks, or 280 days.	A single coitus.
R. R. Pennington.	Forty weeks, or from 280 to 283 days.	Certain sensations of the females.

The extremes therefore, allowed by the witnesses, are from 271 to 283 days.

On the other side, the following medical witnesses gave testimony. Drs. Augustus Bozzi, Granville, Conquest, Blundell, Merriman, Power, Hopkins, Dennison, H. Davis, and Elliotson, and Messrs. Sabine, Chinnocks, and Hawkes.

Dr. GRANVILLE testified that, in his opinion, the *usual or ordinary* period of gestation, is comprised betweeen the 265th day subsequent to impregnation, and the 280th, or forty weeks; but he believed that gestation might be protracted. The most prominent case mentioned by him was one, for whose introduction, the individual concerned, has no reason to thank him. It was that of his own wife. She *passed* her menstrual period on the 7th of April, and on the 15th of August (four months and seven or eight days) afterwards, she quickened. Labour was expected in the early part of January, and accordingly pains came on, but they again subsided, and the female was not delivered until the 7th of February; that is, 306 days, if we calculate from the day before the next

expected menstruation, or 318 days, if we calculate from the middle of the two periods.

Dr. Granville also stated, that he was attached to two of the most extensive Lying-in Institutions in London, and had seen much practice in them; had particularly and carefully registered cases, taking all the leading circumstances of their history from the individuals admitted, on their presenting their letters of recommendation. According to these registers, he had "known a case of 285 days, from the latest period of supposed impregnation, taking as the point of departure, the last day of the month previous to the missed period; that is to say, 28 or 30 days after the last menstruation; also cases of 290, 300, and 315 days." In this last, however, there was a doubt in his mind respecting the report made by the woman. She stated that she had terminated her menstruation previous to falling with child on a particular day; but on cross questioning, she admitted, that for five days afterwards, she continued to have a discharge. In Dr. G's opinion, this was only a case of 310 days.

In answer to the question, whether he believed it possible, that a child should be begotten on the 30th of January, and born at an interval of 311 days, viz. on the 7th or 8th of December, he said, *I am aware of no circumstance that can render it impossible.*

On what may be called his cross-examination, the registered cases above noticed were more particularly inquired into; but, unfortunately, technical difficulties were interposed concerning the character of the testimony, and nothing satisfactory was deduced. It appeared, however, that in one case, which seems to have been of 282 days, Dr. G.'s opinion was drawn from the time at which the female stated she had last menstruated; and the calculation was made from the day previous to the expected, but *passed* period. He also offered a witness in *propria persona*. This female (Mary Parker) had not menstruated for eight months, from the 7th of April, and "we are now (he adds) on the 4th of July, and she carries her child yet." We shall hereafter mention the examination of this person.

Dr. CONQUEST had practised for thirteen years. The majority of cases are completed within the ninth calendar month, but he certainly had met with instances which have far exceeded that date. In not fewer than twenty cases, there had been very confident assertion on the part of the women, that they have exceeded the ninth month; and in two or three cases he had taken great pains to satisfy himself, and was very confident. In one female, who was so certain of her being confined at the anticipated time, that she had her nurse in the house, the period was exceeded by nearly *five* weeks. This female had borne six children. "At that time (says Dr. Conquest) I disbelieved all the cases I had previously heard: I had been in the habit of laughing at them as a public lecturer; but so strong was the evidence, from the most minute investigation of this case, that I was compelled to admit the accuracy of this woman's statement, and my former convictions were very much shaken." It is very remarkable, that at her subsequent confinement, this female again exceeded her calculation by *four* weeks.

In another instance, a lady who had borne nine children, and who, on *some account or other*, had been able five times to determine exactly the day on which she should be confined, and whose predictions had been verified in those cases, exceeded the time by a month and two days. She brought forth the largest child Dr. Conquest had ever seen, after a very protracted labour.

On inquiry as to the probable causes of protracted gestation, Dr. C. stated that he had seen cases in which an occasional loss of blood during pregnancy appeared to interfere with the process. He believed that eleven months had been exceeded. Mental emotions will also protract the period.

On cross-examination, Dr. Conquest stated that his calculation as to the time of birth was founded on the time of *quickening*. He considered this much more certain than that from menstruation. Quickening takes place from the sixteenth to the twentieth week; but when once a woman has quickened at a certain time, then, he believed, that with scarcely an exception, she invariably quickens at the same period afterwards.

Now in the females mentioned by him—the first had quickened with six children exactly at the termination of the sixteenth week, reckoning from the non-appearance of the menstrual discharge, and the period when she supposed herself to become pregnant. "This woman is an excessively irritable woman, physically and mentally; and she affirms most confidently, that she invariably suffers much constitutional disturbance within one week after impregnation, and that the acts of intercourse are so seldom with her husband, that she has in every case been able to date with correctness, with the exception of the two (protracted) cases, and then she took the same data as the ground of her opinion."

In the second case, the opinion was deduced from the absence of menstruation and quickening. She quickened at the seventeenth week, and twenty-eight weeks from that to birth made forty-five weeks.

Dr. Conquest was asked whether he had known a woman menstruate during pregnancy. He replied, "I think a woman does not menstruate, in the common acceptation of the term. I know a woman will lose blood periodically, but I believe these are all cases in which the extremities of certain arteries terminate below the uterus, in the upper parts of the vagina; and I believe, that in by far the majority of cases of reported menstruation, if the discharge is examined by one or two tests, it will be found to be blood, and not the menstrual secretion, which differs materially from blood."

Dr. Blundell had personally known but one case in which pregnancy was prolonged beyond nine calendar months. This female became pregnant on the 9th of August, and was delivered on the 23d of May (287 days.) Dr. B saw her a few days after impregnation—there were symptoms of irritation about the bladder and adjacent parts, and the catamenia were absent. He had no doubt that these symptoms arose from impregnation.

This witness professed himself a believer in protracted gestation, from this case—from the experiments of Tessier on brutes, showing that it actually occurs with them—and the observations of others on the human subject.

Dr. MERRIMAN had practised Midwifery for thirty years. The ordinary period of gestation is about 40 weeks, but in his own experience he has known cases to exceed this—some 285 days, some 287, two or three 296, one 303, and one 309 days. The last was of a lady who had borne six or seven children. "She always calculated her reckoning from the last day on which her monthly period ceased. On this occasion she was perfectly well on the 7th of March; and from some circumstance, which I did not press to know, she said she supposed herself to have conceived on the 8th of March." This lady was delivered on the 11th of January, being 309 days.

On cross examination, Dr. Merriman was asked how he had calculated his protracted cases, he answered. *From the time at which the last appearance of the menstruation ceased; from the termination of the monthly period.* In the last case, the female had menstruated on the 7th of March; and both females were married and lived with their husbands. It was very properly asked, whether the intercourse which produced conception, might not have been at any time previous to the next period; and if so, whether allowing it only to have occurred in the middle between the two menstruations, most of the cases would not be brought to the usual term of forty weeks, while the rest might be referred to it, by admitting the opinion that pregnancy took place just before the expected menstrual period? Deduct 28 days from 309, and the result exceeds forty weeks by only *one day;* Dr. Merriman readily allowed the correctness of all these inferences. He threw out an idea, that impregnation is by no means so common the day before the expected term of menstruation, as it is the day after the menstruation has ceased.

Dr. POWER had practised midwifery for thirteen years. He is decidedly of opinion, that gestation may be extended to eleven calendar months, if not longer. He had met with from thirty to fifty cases, in which it exceeded the ordinary term, and some in which it went to the period just named. His opinion is deduced from the statements of the females,

as to the period of menstruation, and the time of quickening, and also from physiological reasoning.

The latter presents some interesting points, which it may be proper to mention. Labour, according to Dr. Power, is excited in consequence of the contents of the womb being brought into immediate contact with its mouth; and the neck has been intended to keep off labour until it has been obliterated, and until the child is perfected. At the end of utero-gestation labour takes place, in consequence of the stimulus applied to the mouth of the womb. The uterine tumour has also subsided immediately previous to labour, and thus furnishes the stimulus. Now any causes that could prevent the pressure in question would retard it. Among these are insensibility of the os uteri; and in one protracted case, probably of this kind, Dr. Power brought on labour immediately by applying a bandage around the abdomen. Another cause may be, the position of the mouth of the uterus situated at its side, instead of directly in its centre.

Dr. Power mentioned a case in which labour came on, the membranes ruptured *and the labour was deferred for nearly a month.* It is well remarked on this, by Dr. Lyall, that "such cases must be very rare, and that some (he might have added *many*) practitioners and lecturers think such an occurrence impossible."

Dr. Hopkins is in extensive practice, and has attended about one thousand cases annually, during the last five years. The ordinary period of gestation is about 280 days; but he believes it can be protracted; he has known one positive case of this nature. Unfortunately, Dr. Hopkins did not precisely remember the particulars, but from what he did mention, it would seem that he was consulted *some time* in December: the abdomen was very much enlarged, and from every appearance, it was a four months' gestation; but delivery did not take place until the 4th of June, being according to the witness, a case of *ten calendar* months. It was eleven months and a day since her last menstruation, and she had quickened fourteen weeks after conception.

Dr. Hopkins had certainly either a very confused recol-

lection of this case, or else it was more than an eleven month's case. He most positively remembered that she last menstruated on the 3d of June, while she was delivered on the 4th of June: i. e. twelve months and a day. From quickening to birth would have been about 29 weeks. Again, he could not tell whether it was on the 1st or 31st of December, or any intermediate time, that he was consulted.

Dr. Dennison merely knew of but one case, where the female went three weeks over the usual time, and he could not speak positively to the fact of that one. His opinion was derived from the narrative of the lady, but he did not make any particular examination. He allowed that females often miscalculate, for a fortnight or three weeks, and he had formed no judgment on a case of 311 days.

Dr. Henry Davis had practised midwifery twenty years. In his own experience, he had met with but one case of protracted gestation, and then he was led to believe it was owing to some mistake of the patient. A remarkable instance had, however, come under his observation; but on commencing its recital, it was ascertained that he had learnt the facts from others. *Hearsay evidence*—that aversion of the lawyers, could not be admitted, and he was dismissed.

Dr. Elliotson had delivered a course of lectures on forensic medicine some years since in London, and from examining for this purpose, the works of eminent persons on the subject in question, he believed that women have frequently gone more than ten calendar months. He had no experience in midwifery.

Mr. Sabine, surgeon and accoucheur, like Dr. Granville, dragged his wife before the committee. Her last menstruation was on the 14th of September; she quickened in the second week of January, and was delivered on the 14th of August—being a ten months' case, if we date from the 14th of October, or, ten months and a fortnight, if from the middle of the period.

On his cross examination, he stated that quickening occurs usually about the sixteenth week, although this is liable to variety in either way.

Mr. Chinnocks related the case of a female, who calculated from the time her husband (a trader to America) had left her. The dates are not specified, and all we can learn is, that she exceeded her calculation eighteen days. It is to be regretted, that the particulars of this case were not more minutely investigated by the counsel.

Mr. Hawkes, an accoucheur at Oakhampton, in Devonshire, closes the corps of medical witnesses. He mentioned some extremely loose stories of women going forty-one and forty-two weeks; but no definite facts were given by him. At the conclusion of this testimony, he *sported* an opinion, that as far as his observations on women and animals extended, they went longer with males than with females. The Solicitor-general, in his cross examination, pursued this idea. Mr. Hawkes supposed 280 days the usual pericd for females, and a week or ten days more, (290) the time for males. But suppose the child is an hermaphrodite, what then is the time? He answered, "*that I should take between the two.*" In reply to a remark of the Solicitor-general, that the people at Oakhampton do not come regularly, he said, "*it is owing to the cold weather!*" Finally, he argued, that when a woman had been mistaken, it was generally a *boy* that was born.

It may be proper also, to compare the periods to which the believers in protracted gestation extend the term.

Dr. Granville says,	313 days,	and judges by the last menstruation.
Dr. Conquest,	315 days,	quickening & menstruation.
Dr. Blundell,	287 days,	experiments on brutes, &c.
Dr. Merriman,	309 days,	last menstruation.
Dr. Power,	11 calendar months,	quickening, menstruation& physiological reasoning.
Dr. Hopkins,	11 months,	enlarged abdomen, quickening and menstruation.
Dr. Dennison,		
Dr. H. Davis,		
Dr. Elliotson		
Mr. Sabine,	300 days,	last menstruation.
Mr. Chinnocks,		
Mr. Hawkes.		

This statement is evidently a very encouraging one. It is a great point gained, to restrict protracted gestation within the year. We can thus sweep off the thousand stories recorded in old authors, to their proper receptacle. They will hardly bear quotation again in a court of justice. But let us notice a few of the female witnesses.

Mary Parker, the witness presented by Dr. Granville, stated that she had been in her present state of pregnancy, eleven months. She had lived with her husband during all this period. She had menstruated last during the latter end of July. She was then suckling her child, and by the advice of a physician weaned it, as he thought she was two months pregnant. When asked at what time she left off suckling, she replied, *nine months ago*. She quickened about 23 weeks since. Comments are surely unnecessary on this case.

Mary Summers, the mother of twelve children, thought she had an eleven month child, because she quickened during Christmas week, and was not brought to bed until the 4th of August. The quickening, however, appeared on close inquiry to have been only a faintness and fluttering, and she could not swear to strong motion until six weeks afterwards.

Several females, who had borne large families of children, were examined to the same effect. They believed that gestation had been protracted to ten months with them in individual cases, and founded their opinions on the cessation of menstruation. Their answers are however very loose, and their recollections of the time of quickening, either very uncertain or altogether wanting. No fact that deserves mention can be drawn from their narratives, except the one just noticed.

On a perusal of the evidence now given, it will be seen that it is the *testimony*, rather than the *case*, which is deserving of examination. The proof of bastardy was so decisive, that the House of Lords readily concurred with its committee in bestowing the title and estate on the son by the second marriage; and it may be questioned whether the medical evidence was much regarded in coming to this decision.

But the discordance of opinion manifested by the medical

witnesses, and their mode of judging from various data, as well as their different modes of obtaining those data, lead to important reflections. I do not intend enlarging on these, and more particularly because the subject has been noticed at large in various European works,* but I will venture a few observations, with a view to elicit discussion.

1. *Must the advocates for a fixed term of gestation prove that labour occurs on a certain fixed day after conception*, or, in other words, that delivery happens on the 273d or 280th day precisely? This would seem to be the main *logical* argument urged against them. You object (say their opponents) to protracted gestation, you do not believe in ten or eleven month cases, and yet you, as in this very case, allow a latitude of from seven to ten days. This objection is rather specious than solid. Neither party have yet agreed on an invariable criterion from which pregnancy is to be dated; and it appears to be sufficient, if cases can be explained, by taking within their range the period between actual menstruation and the next expected, but passed period. The believers in a regular term do not seem to ask even for this. They only allow a variation of seven or eight days; and it must certainly be granted, that at present it is impossible to ascertain the period of conception more accurately. Besides, is it not perfectly within the range of possibility, that all these cases, though thus varying, might, if it was in our power thoroughly to investigate them, have a fixed term, even to days?

2. The variation of the different signs, from whose presence or absence the duration of pregnancy is computed, is so great that an approach to a common ground of discussion seems as distant as it was in the days of Aristotle. Thus, peculiar sensations at the time of conception are much insisted on by some, but these are often undefined and doubtful, and even if remarkable, have not been noted with sufficient accuracy.

---

* See modern writers on Medical Jurisprudence; and with reference to this particular case, Dr. Lyall's Introductory Remarks to the Evidence. Edin. Med. and Surg. Journal, vol. xxvii, p. 109; and Medico-Chirurgical Review, No. ix, 170.

The period of quickening also varies, according to authors, from ten to twenty-six weeks—a period long enough to explain almost every modern case of protracted gestation: and even the idea thrown out by some of the medical witnesses, that "the date of quickening is very uniform in the several pregnancies of the *same female*," although probable, is yet very far from being established by an adequate number of published observations.

But the cessation of the catamenia, the period from which females almost universally calculate, is above all so uncertain a criterion, that we see physicians abandoning it. The menses may be *supposed* to be present, when pregnancy is actually advancing. They may be wanting from disease, and the female compute according to it. According to several writers on midwifery, the inferior portion, and more especially the neck of the uterus, is not for the first two or three months always occupied by the decidua, thus leaving a space capable of secreting a diminished portion of the menstrual fluid. Many observers also, we know, believe in actual menstruation during pregnancy. A colourless fluid is also spoken of, as occasionally discharged, and which may certainly be present during the early period of these supposed protracted cases.

Besides these sources of fallacy, Dr. Lyall has indicated another, on the authority of an eminent teacher of midwifery, as to computation. "It is assumed by the medical witnesses, that 28 days intervene between the end of one menstrual period and the commencement of another; whereas every woman could tell them that 28 days comprehend a period and the interval; or in other words, if a woman begin to be unwell on the first of May, she will be, if regular, again unwell on the 28th of that month."* Hence a healthy unimpregnated female will have thirteen menstrual periods during a year, and ten during a period equal to that usually occupied by preg-

---

* Lancet, x. 660. [Note.] The suggestion of Dr. Lyall, that all our computations should be by *days*, seems a very necessary one.

nancy, viz. 280 days. If she then calculates by lunar months, (28 days) we shall have a case of protracted gestation.

As to expansion of the abdomen, or even the touch, (*ballotement* of the French) these are so very uncertain, that they hardly require animadversion. Dr. Collins,* in what he calls an eleven months' case, has much insisted on the value of the latter; but we observe that his narrative does not find great favour, even with Drs. Johnson and Lyall, who are both advocates of the possibility of a *moderate* protracted gestation.†

A single coitus would seem the most unexceptionable era, if we could satisfy ourselves as to the character of the female. But with married females, this is generally inapplicable; and with unmarried, (the victims of seduction) doubt will always arise. They are interested in referring their case to a single act.

The absence of the husband, (as in this present case) or his death, has been the fruitful source of many trials on this very point; and here, certainly, it is not far from truth to say, that some have been marked with every feature of unblushing whoredom. The very fact of their being brought into court taints them. Dr. Dewees in his work on midwifery, mentions the following, which is far removed from the eleven and twelve and thirteen months' cases of continental Europe. "The husband of a woman, absent several months in consequence of embarrassments, returned clandestinely one night; this visit was known only to his wife, his mother, and Dr. Dewees. She was delivered of a healthy child in nine months and thirteen days. She was within a week of her menstrual period, which was not interrupted, but the next one was."‡ Now if we include February in these nine months, the term will be 283 days; if not, 285 or 286 days; not forty-one weeks.

3. If such uncertainty rests on the subject, is it not more

---

* Edinburgh Med. & Surg. Jour. 25. 245.

† Medico-Chirurgical Review, 9. 172. Dr. Lyall's Introduction, p. xix.

‡ Dewees' Midwifery, p. 170.

philosophical to embrace the "*common sense*" view of the question—the prevalent opinion of all ages and all countries—verified by an immense majority of cases occurring every day, and strengthened by the powerful fact, that the dead fœtus and twins are delivered at the usual period? If not, let its opponents bring more unexceptionable cases (excluding now the idea of vice) before the medical public. Let the recollections of females be more accurate, and the data on which an opinion is to be formed, more unquestionable.

ART. V. *Case of Stricture of the Rectum and Colon.* By CYRUS PERKINS, M. D. of New-York.

THE subject of the following case, one of our most valued citizens, was 48 years of age, spare in his person, of vigorous intellect, and sedentary habits.

In the month of August, 1826, he complained to me of a difficulty in evacuating the fæces; which were passed either of a very diminutive figure, or in a liquid form only; and often required repeated efforts, each day, to render the state of the bowels in any degree comfortable. He was occasionally relieved by such mild purgative medicines as quickened the motion of the bowels, and produced liquid stools. But the difficulty soon returning, I suspected the existence of stricture, and endeavoured to ascertain the fact, by introducing my finger up the rectum; but was unable to reach the seat of disease. I then proceeded to examine the parts with a bougie, prepared by strips of linen wound on a flexible piece of whalebone; the strips being first dipped in a composition of wax and lard, melted; and, to render the instrument softer, it was, at last, repeatedly immersed in the same composition, till brought to the proper size.

On introducing this flexible bougie, which was nearly three fourths of an inch in diameter; a resistance was perceived five inches up the rectum, which seemed indeed, at first to terminate in a blind pouch; but by changing the direction of the instrument repeatedly, guided in part by the hand and feelings of the patient, it was carried up, evidently through

a strictured portion of the gut, two or three inches higher, till there seemed to be no further resistance. This operation was repeated, for several successive days, with the evident effect of dilating the rectum in the straitened part. Still the patient was not essentially relieved; and the conclusion was inevitable, that a stricture was to be sought for higher up. Recourse was, accordingly, now had to a longer bougie, prepared in a similar manner, but smaller in size, and tapering to a point, somewhat less than one third of an inch in diameter. After repeated unsuccessful attempts, this instrument, guided as before, principally by the hand of the patient, was at length carried up nearly twelve inches, where its further progress was arrested. It was retained in this situation till it occasioned considerable uneasiness, when it was removed, and was immediately followed by a slight discharge of fæces. On examining the bougie, it appeared that the point, which had taken a short turn toward the left of the patient's body, was strongly impressed by a stricture; and that the body of the instrument, had also the impression of two contractions of the rectum lower down; the one about five, and the other seven inches from the anus. At these places the instrument was flattened, as though bands were drawn across the rectum. It was thus rendered certain, that there existed three strictures, the upper and principal one in the sigmoid flexure of the colon, and at such a height as to render its treatment by bougies embarrassing at least, if not dangerous. It was repeatedly attempted to dilate the stricture, by the pressure of water contained in a portion of sheep's gut. But we entirely failed in effecting our purpose, from being unable at such an unmanageable height, to introduce the gut sufficiently into the strictured part. The wax bougie, prepared as before described, and made quite flexible towards the point, was the only mechanical contrivance we could render useful. During the progress of the disease, the patient frequently complained of pain about the upper stricture, oftentimes very severe. This, as he believed, occurred whenever feculent matter, or even wind, passed the strait. This pain was always mitigated by pushing up the bougie, so as to produce

gentle distension of the bowel, at or near the stricture; and by the same means, the passage of feculent matter was promoted. So sensible was the patient of the relief thus afforded him, that he often retained the bougie for several hours together; flattering himself that what was now merely yielding him a respite might, by perseverence, work a permanent cure. The immediate and uniform effects of the bougie, made it evident that some of the most distressing symptoms of the disease, arose from a spasmodic state of the muscular coat of the intestine; occasioned, no doubt, by an irritation arising from the permanent stricture. The occasional severe pain, was unquestionably from spasm; and the relief afforded by the bougie, was from its gently distending force subduing an inordinate contraction of the muscular fibres; on the same principle that spasm, and the pain arising from it, are always relieved, whenever pressure can fairly be applied to the part affected. The bougie procured stools, in this case, by fatiguing the muscular fibres below the stricture, and thus bringing those immediately above it, by continuous sympathy, into the same condition; so that, for the time, they could present no further impediment to the ordinary function of the bowel.

For months the bougie was daily resorted to. In some instances the patient retained it for many hours together, and generally with uniform convictions of its utility. So that he sometimes flattered himself that the disease was gradually yielding. He was at length, however, repeatedly admonished of the deception, by indulging too freely in his diet. Severe pain, obstinate constipation, and sometimes vomiting, during the last two or three months, had several times threatened a sudden and fatal termination. In the progress of the disease, I had occasion to prescribe a variety of medicated suppositories and enemas, sometimes with a view to evacuate the bowels, and at other times, for the purpose of allaying irritation and spasms. The tobacco in infusion, as well as in the form of suppositories, I often found useful in answering both these indications; but it became inconvenient, from the difficulty of controlling and graduating its effects.

For the last six or eight weeks of this case, it became ne-

cessary to resort to pretty active cathartics. Salts and senna, or castor oil, were taken as often as every third or fourth day. The latter, on the whole, was found preferable; and was taken in such doses as to produce pretty copious stools. But whatever cathartic medicines were administered, considerable pain was experienced about the loins during their operation; and a good deal of irritation and uneasiness remained there, and in the course of the rectum, for some time afterwards. To relieve these distressing symptoms, various expedients, such as fomentations, tepid bath, &c. either afforded but partial relief, or totally failed, till I resorted to opiate suppositories, which were uniformly attended with most happy effects; not only terminating, as by a charm, all these pains, but at the same time, putting to rest a most uncomfortable sensation, often complained of, about the perinæum and glans penis. The quantity of opium necessary for this purpose, was from one to three grains, according to the urgency of the occasion.

My patient early learnt the importance of a rigid adherence to the course of diet I had pointed out. Which was to avoid much solid food, and depend principally on gruels, beef, and chicken teas, broths, soft eggs, jellies, fresh fish, &c. His drink was to be toast-water, barley-water, black tea, &c. avoiding all ardent spirits and wine. But, debilitated by a constantly irritable state of the system, and a consequent want of regular and quiet rest—emaciated too by the same cause, by anxiety of mind and spare diet, he sometimes reasoned himself into the belief, that it was not only safe, but expedient to indulge occasionally in a little more substantial food. These impressions having taken root, and at the same time, urged on by the solicitations of a craving appetite, his reasoning would now and then result in a good dinner. Such had repeatedly been a cause of much suffering, and such was the cause of the last fatal attack. Within an hour after eating a dinner of solid food, and rather indigestible in its kind, he was seized with the most frightful spasms of the intestines; and I found him soon after, literally writhing in agony.*

* Mr. White, of Bath, in his valuable *Observations on strictures of the*

This was on the 31st of March last, at 4 P. M. For immediate relief, under circumstances so urgent, I resorted to liberal doses of laudanum, large anodyne fomentations to the abdomen, and warm bath; which, in the course of two hours had diminished, somewhat, the acuteness of suffering; still the pain was great, and the abdomen had become considerably swollen and tense. In the evening a medical gentleman was desired to attend in consultation. The patient was now more free from pain, but could not be moved without the recurrence of severe spasms. He had not been able to lie down for the last five hours, nor change his posture in any way from the position in which he at first placed himself; sitting in an armed chair, with his body bent forward, and compressing his bowels with both hands. He was, at length, removed to bed with great difficulty, and considerable increase of pain. An opiate was now directed, and the fomentations to be continued through the night, as occasion might require.

April 1st. The patient had passed a restless night, with little sleep. The abdomen was tense, with considerable soreness. The drinks, which were toast-water and barley-water, during the night, had sometimes been rejected from the stomach. And during the day, the patient remained in so irritable a state, as to reject every thing, except a weak alkaline solution. Fomentations were repeated, and enemas.

April 2nd. The last night was passed without much repose, though a large anodyne had been administered. The stomach, however, had become somewhat more retentive. Two other gentleman were joined in consultation; and, as there had been no evacuation of the bowels for three days, castor oil was prescribed in repeated doses, to be aided by stimulating enemas. The oil was retained till several doses

---

rectum and colon, p. 83. observes, that he has reason to believe, that two patients with strictures, lost their lives suddenly by a similar indiscretion. "Eating and drinking more freely than ordinary one day, they were seized with symptoms of iliac passion, and both of them died in a short time; no passage through the bowels could be obtained."

had been taken, but without effect; and the clysters were returned unaltered. The stomach was again more disturbed, and a large epipastic was applied. During the evening and night, the patient was distressed by obstinate hickup and occasional vomiting; and a dark fluid, resembling coffee, in colour, was thrown up in considerable quantities.

April 3d. The plan which had been adopted having entirely failed, it was decided to commence the use of calomel, 10 grains every two hours. This was pursued during the day, but without producing any relief. At evening, the hickup was almost unremitting, and the vomiting had increased. The ejected fluid was darker, and contained numerous brown membranous flocculi. An anodyne was prescribed, and I left the patient at 12 o'clock, more composed, but evidently sinking. At 2 o'clock, on the morning of the 4th, I was called to him; and, on informing him of the near approach of death, he expressed great satisfaction that he was so soon to be released from his sufferings. He expired at 3 o'clock.

At nine o'clock, six hours after death, the body was examined in the presence of the gentlemen who had been in attendance. On laying open the cavity of the abdomen, the viscera were found completely glued together, by recently effused lymph, and generally adhering to the peritoneum. There were several pounds of turbid serum, and a considerable quantity of well-formed pus. The small intestines were distended with flatus, and the colon with scybala of various size, and prodigious hardness. This intestine was, in no part, enlarged; and, from the left extremity of its arch downwards, was preternaturally contracted.

Looking into the pelvis, the bladder of the urine was seen empty, and the rectum diminutive in size, and snugly trussed down to the bone by a thickened peritoneum; not passing up the centre of the hollow of the sacrum to its promontory, but taking a course obliquely to the left of the patient's body, up the sacro iliac symphysis.

At and somewhat above the brim of the pelvis, the colon was found to be contracted to a very small size. Here was

the principal stricture. It felt like a hard cartilage, for a about two inches in length, and was closely and firmly bound down by a morbidly thickened peritoneum. The vestiges of high inflammation were almost equally conspicuous throughout the whole cavity; and a fluid was seen oozing from about the centre of the stricture; so that, at this point, it was either actually ulcerated before the examination, or was in so tender a condition, as readily to give way in attempting to remove a portion of the intestine from the body. On examining the part after its removal, the two lower strictures, or rather contractions, in the rectum, were found to be produced by a thickening and tension of the peritoneum merely, over its anterior surface; whereas all distinction of coats seemed to be lost, at and about the upper and main stricture, except the villous, which was thrown into irregular folds, and could readily be expanded. The calibre of the bowel at this part, was reduced to about one third of an inch in diameter. The peritoneum, with the other coats, and some portion of the cellular membrane around, were assimilated, thickened, and indurated; constituting, so far as I could judge, a carcinomatous state of the parts. Indeed, there seems to me, sufficient reason for doubt, whether there be any just ground for distinction, between what has been denominated the indurated stricture of the rectum and colon, and the scirrhous stricture; which are admitted by writers of the most extensive observation, to be "apparently similar."* As to a diagnosis founded on the circumstance of "pressure of the bougie being generally useful in the one, and inadmissible in the other," it seems to me, quite vague and unsatisfactory.† These circumstances may depend merely on the stage of the disease. While the parts remain nearly insensible, the bougie affords temporary relief; but when a preternatural sensibility has been awakened, the bougie becomes inadmissible.

During the attendance in this case, it was proposed in consultation, to cut down on the colon in the left iliac region,

---

* See Calvert's excellent Practical Treatise passim; et al.

† Idem, p. 132.

and endeavour to prolong the patient's life by establishing an artificial anus. This was advocated on the ground, 1st. That the case was otherwise desperate. 2nd. That the operation might afford some hope.—That it was practicable; that cutting into the abdomen had been repeatedly done without serious consequences; that it would be more likely to diminish than increase the inflammation; that possibly the stricture might be acted on mechanically, from the opening in the colon above; and that, though an artificial anus would be a serious reduction of the value of life, the duty of medical men required them to do all that could afford a rational prospect of prolonging it, even on the worst terms. The proposal, however, was opposed on various grounds, and overruled. It was considered, 1. That the state of the intestines, and the situation, extent, and nature of the stricture, could not be sufficiently known to warrant the measure. 2. That the symptoms of a high state of irritability and inflammation, already present in the abdomen, admitted no probability of success. 3. That patients do not long survive an artificial anus, under circumstances incomparably more favourable than the present: and lastly—That, should the operation succeed, against all other objections, it could prolong life on such conditions only, as would render it burthensome to the possessor.* These considerations appeared to me decisive at the time; and in my judgment, were strengthened by the post mortem examination. The proposal of such an operation, I am persuaded, can never be received with favour. A patient would not, and ought not to submit to it, so long as a ray of hope remained of living without it; and when this was extinct, the case would be desperate in every view. I cannot therefore but express my conviction that a disease, like the above described, will never submit to the control of art. It is a calamity from which there is no refuge but the grave.

P. S. *June 18th.*

Since writing the above, I have been called to visit in consul-

---

* On consulting the patient, this last objection was, in his mind, insuperable.

tation, with Dr. H. A. Riley, Mrs. C—, who had for some days been labouring under the urgent symptoms of constipated bowels. There was great pain, tension and soreness of the abdomen, with nausea and vomiting. During the three days I attended this patient, the most active treatment proved unavailing. For the last twenty-four hours, a dark fluid was thrown from the stomach without nausea. She expired yesterday. On examination, soon after death, the stomach was inflamed, and nearly the whole tract of small intestines, from the duodenum downwards, was in a state nearly approaching sphacelation, but there were no adhesions. The colon was inflamed, and greatly distended, from its head to a point just above its sigmoid flexure, where it was suddenly contracted and indurated, constituting a stricture which had evidently been of long standing. Its structure, in my judgment, was scirrhous, and similar to that in the case above related. The adipose membrane, in contact with the part, had become involved in the disease; and the lower part of the strictured portion was ulcerated, so as to allow feculent matter to escape into the general cavity. The bowel below the stricture was preternaturally contracted.

ART. VI. *Case of Malformation of the Heart.* By RICHARD K. HOFFMAN, M. D. of New-York.

THE subject of this deviation from the natural structure of the heart, was born in June last, and presented no remarkable phenomenon, either immediately subsequent to his birth, nor during the ensuing warm months; on the approach of winter, he was observed to be very sensible to the impression of cold air; and his lips, nails, eye-lids, and superficial veins, assumed a preternaturally dark, or blue-black colour. In December I was sent for on account of his appearance; his general aspect was healthy, but diminutive for his age; and as this discoloration was of recent origin, it at first occurred to me, on examining the dress, that the tightness of the bandage encircling his body, impeded the descent of the diaphragm, and the free expansion of the lungs—and this, in a

degree, prevented that change of colour which the blood should undergo in its circulation through the lungs. The experiment of substituting a loose dress was made, which showed the error of the above reasoning, and led to the conclusion, that some defect existed at the source of the circulation. The action of the heart examined externally —the pulse—respiration—and temperature of the surface of the body, were not affected in any sensible degree; if at all, the pulse at the wrist was stronger, and the skin cooler to the touch than natural. It was evident that the child possessed, in a diminished degree, the power of generating heat, from the extraordinary quantity of clothing required to keep him comfortably warm. He seemed impatient of the usual exercise to which infants are accustomed; and when left to his own volition, showed but little disposition for any muscular exertion. During the present month, he was attacked with whooping-cough; a paroxysm of which was succeeded by fits, with great turgescence of the face and coma, as in apoplexy. Under these circumstances, the temporal artery was divided, and the blood that was freely discharged from it, was of a very dark colour, without any of the florid appearance of arterial blood; a temporary relief of the urgent symptoms followed, with a cessation of the convulsions and restoration of sensibility; his appearance for a few days encouraged a hope in his relatives of his ultimate recovery, when after a violent effort of coughing, he died in the ninth month of his age.

On examining the heart, the right ventricle was found imperfectly developed, and opening directly through the imperfect septum into the left ventricle; besides this communication between the two sides of the heart, there was another still more direct, between the sinus of the *right auricle and the left ventricle*, through which the finger could be introduced; the foramen ovale was also pervious, and the sinus of the left auricle small in proportion to the right; and a striking peculiarity was, that both the aorta and pulmonary artery, arose from the left ventricle; and from the relative size of the arteries, it appears that scarcely one-fourth of the

mass of the blood was sent to the lungs, to undergo the requisite change from the influence of the air in respiration, which accounts for the dark hue of the blood drawn from the temporal artery, &c.

The communication between the right auricle and the left ventricle being so direct, what structure is there to prevent the regurgitation of the blood into the sinus of the auricle, during the contraction of the ventricle? is not one of the mitral valves so disposed as to have this effect? or the left ventricle, having both arteries arising from it, was the vis a tergo from the column of the returning blood in the venæ cavæ, sufficient to prevent its reflux from the ventricle?

ART. VII. *The History of a case of Strictured Vagina preventing delivery. In a letter to* Dr. J. B. BECK. By JAMES MCNAUGHTON, M. D. Professor of Anatomy in the University of the State of New-York.

WHILE attending the meeting of the State Medical Society, in this city, in 1826, I made you, in some degree, acquainted with an interesting obstetrical case, I then had under my charge. The result was not determined when the session of the society closed, and you expressed a desire to know how the case terminated. I now avail myself of a few moments of leisure, to give you a brief sketch of its history. Cases like the one about to be related are of rare occurrence, particularly in this country; and when they are met with, are always very trying to the feelings of the practitioner, inasmuch as the life of both mother and child may depend upon the course he pursues. It frequently happens also, that one life must inevitably be lost; and there may be peculiar circumstances rendering it a nice moral question, which life ought to be preserved, if one *must* be sacrificed.

The unfortunate subject of the following history is of small stature, but of good constitution. Some nine or ten years ago, she married, and after an ordinary lapse of time, fell in labour of her first child. It proved very tedious; and from some cause or other, she came under the care of several prac-

titioners, not in consultation, but separately, before the termination of her labour. The gentleman last called to see her, found that she could not be delivered without the use of instruments, and that the life of the child, if not already lost, would be endangered by the process. He, therefore, called in another practitioner of greater experience and years to advise with, as to the course that ought to be pursued. At the time of this consultation, the head had been for more than twenty-four hours resting upon the external parts, or at all events, so situated, as to make injurious pressure upon the vagina, bladder, and labia. It was agreed upon to apply the forceps; this was accordingly done. A still-born child was brought into the world; but in doing this, the mother suffered severely. The perinæum was lacerated, and such a degree of inflammation followed the birth of the child, that a large portion of the base of the bladder, the whole anterior part of the vagina, and perinæum, sloughed away.

As might have been expected, the recovery from so severe an injury, was extremely slow, and attended with great pain, and much inconvenience on account of the constant escape of the urine through the opening in the base of the bladder. During the time of the surgeon's attendance, precautions were taken to prevent the adhesion of the opposite sides of the vagina, but these measures being afterwards discontinued, the cicatrix contracted so much as to leave only a small opening, just sufficient to permit the catamenia to pass.

Eight years had now elapsed from the birth of her first child, when, very much to her own surprise, she found that she was again pregnant. Her gestation went on regularly until February, when she was attacked with the influenza, then prevailing as an epidemic. On the 8th of the month, labour pains came on, and I was sent for; but, being at the time so engaged that I could not attend, I requested that my friend Dr. Eights might be sent for, to take charge of the case. He was accordingly called in. As soon as I got disengaged, I called at the house to ascertain whether she had got a physician, and found Dr. Eights still in attendance. Soon after entering the room he told me that it was a

dreadful case; and that he did not see how delivery was to be effected, unless by the cæsarian operation. I immediately made an examination and found the case bad enough; much worse than I had anticipated, although prepared to meet with considerable difficulty. About two inches from the orifice of the vagina, my finger encountered a firm substance, having nearly the hardness of cartilage, which closed the passage almost entirely. At the upper part of the cicatrix, and behind the symphysis of the pubis, there was a small depression, which seemed to be the opening of a narrow passage leading to the uterus. The cicatrix had a puckered appearance, and had numerous firm bands, running from the labia and perineum towards the opening, in the form of rays. Presuming the opening to be a passage leading to the uterus, I tried to introduce a probe into it, which I succeeded in doing; but from the obliquity of the canal, I found it difficult to ascertain the state of the vagina above the cicatrix. Moreover, from its firmness and solidity, and the information conveyed by the probe, I was satisfied that the contraction was of considerable width, if it did not extend all the way to the os uteri.

In this stage of the labour we did not deem it necessary to interfere: we, therefore, told the patient we must wait to ascertain what effects the pains would produce upon the obstruction, before we proceeded to active measures for her relief. This being late in the evening, we took leave, and saw no more of her until next morning after breakfast. We then found that the pains had been quite regular during the whole night; but not the slightest change had taken place in the cicatrix. Supposing the pains to have been genuine, we felt assured, from the slight impression made upon the cicatrix, that nature alone was inadequate to overcome the obstacles to delivery, and that some assistance must be given.

A consultation of several physicians was called, and among the number, the two gentlemen who had had charge of her in her first confinement. A slight examination was sufficient to show that the bony outlet of the pelvis was unusually narrow. This alone, even if there were no cicatrix,

would have caused much difficulty in accomplishing delivery, and probably was the cause of the severity of her first labour. The difficulty was greatly increased by the callous and contracted state of a considerable portion of the vagina. Some, if not all, were firmly of opinion, that a living child could not be brought through the natural passage by any operation that we could perform. Some contended, that, from the condition of the parts, even a dead child could not be delivered without great injury to the bladder, vagina, external parts, and perhaps the rectum; and that in the then state of her health, there was hardly a probability that even her life could be saved. To attempt to deliver the child through the natural passages, it was thought would be certain destruction, while the life of the mother would be brought into great jeopardy. Influenced by these considerations, some of the gentlemen were rather inclined to resort to the cæsarian operation, by which there was a probability of saving the child, and at least a *chance* of saving the mother.

There being a difference of opinion as to the best course, it was agreed to take time for deliberation, as the case was so important. It was, therefore, determined that we should again meet at 3 o'clock, P. M. and that all the necessary arrangements should, in the mean time, be made for performing the cæsarian operation, should it be the sense of the gentlemen present that it was expedient, so that no time might be lost. At the appointed hour I attended, prepared, but with much reluctance, to perform this horrible operation. As this consultation was to be final, I again made another, and a more critical examination of the state of the vagina. A small round catheter was at first with difficulty introduced through the opening of the cicatrix, and gently pushed towards the uterus. It being of the utmost importance to ascertain, whether the vagina was contracted all the way from the opening in the cicatrix to the os uteri, I bent a small probe, about an inch from its extremity, and entered it into the passage through the cicatrix; I then introduced my finger into the rectum, and carried it up so far, that I could feel

the probe beyond the contracted part of the vagina. I then laid the bent probe flat upon the back part of the vagina, and freely moved it up and down, and was able to feel it distinctly moving over the finger placed in the rectum. The cicatrix as felt from the rectum, appeared to be from an inch to an inch and a half in thickness, and had the same firmness nearly as it presented in the vagina. Where the contracted vagina had the passage leading to the uterus, it did not appear that the cicatrix was so broad as towards the rectum and the sides of the vagina; but in every situation in which it was examined, it presented the same semi-cartilaginous hardness.

By the freedom with which I was enabled to move the curved probe, and by the natural feeling of the parts above the callous cicatrix, I was satisfied that the vagina above was sound, and without contraction; and as the head of the child had not yet descended so low as to press against the cicatrix, notwithstanding the regularity and long continuance of the pains, I felt assured that they had been false, and that labour had not, in all probability, yet fairly commenced; and as she was (as has been already mentioned) labouring under the influenza, my suspicion was rendered more probable. The other gentlemen agreed in this opinion; and as there was pain in the breast, cough, and a pretty free pulse, it was thought expedient to take some blood from the arm, both with a view of lessening the general symptoms and the labour pains. The venesection was followed by an ordinary dose of Dover's powder. This procured her a night's rest, and the pains did not return again with any regularity for several days; but the influenza still continued, though with less severity than at first. On the 19th the pains again returned with severity and regularity. Some of the gentlemen at first consulted were again called. It being now satisfactorily ascertained, that the vagina above the cicatrix was sound, it was deemed expedient to divide the cicatrix to a certain extent, so that the state of the os uteri could be ascertained. I therefore took a curved grooved directory, and passed it through the small opening; and then took a curved bistoury, having the cutting edge covered with linen to within an inch

of its point, to prevent injury to any part but what was intended to be divided. The cicatrix was then divided on each side, carefully avoiding the bladder on the one hand, and the rectum on the other, until an opening was made large enough to allow my fore-finger to be passed through to the os uteri. This enabled us to ascertain with accuracy the state of the labour. The os uteri was dilated to the size of half a dollar, with its margin thin and easily dilatable. It was thought most advisable not to divide the cicatrix to a greater extent than had already been done, trusting that the labour pains would prove sufficient for its further dilatation. The risk of wounding the bladder or rectum, could in this manner be avoided. The labour was accordingly allowed to proceed. When the head of the child began to press firmly against the cicatrix, I applied the edge of the bistoury during the pains to different parts of its circumference, where it seemed to afford the most resistance. I found by dividing in this manner, to the extent of a line or two at several distinct points, that the pains produced much more effect in dilating the callous part. The labour went on in a very regular manner for about twenty-four hours; and the head had descended so low, that I had very sanguine expectations, that the woman might yet be delivered of a living child.

The face was directed towards the left sacro-iliac junction, and had come down to where it takes the pivot turn upon the spine of the ischium; but here, unfortunately, it became locked, and the pains produced no effect upon it for four hours. The pains were gradually becoming more and more feeble, and the mother began to show symptoms of sinking. Having been up all night with her, and being myself sick with the influenza, I became nearly exhausted with fatigue, and felt no longer equal to the sole management of the case, I therefore requested that Dr. Eights might again be called in. Which was immediately done. Upon making an examination, he indulged the hope that the natural efforts might yet prove adequate to the delivery of the woman. We allowed the pains to go on for two hours longer; but no change having taken place, and the pains lessening both in

frequency and force, the doctor became convinced that no good could be expected from the pains alone. There being no space for the application of the forceps, I applied the vectis over the ear next to the pubis, and made exertions during each pain, with all the force I could prudently exert, to bring down the head; but after persevering for three quarters of an hour, without effect, and feeling fatigued, I gave the instrument to Dr. Eights. He made powerful efforts during each pain, but with no advantage. During one of these efforts the cicatrix, where it was connected with the left labium gave way, and a considerable laceration of the inner side of the labium took place, followed by a copious hemorrhage from the torn vessels. Having both given the vectis a fair trial, and found it ineffectual, we suspended further proceedings with it. The only alternatives now left us were, to try the forceps, or to open the head. The forceps could not be applied without further laceration of the external parts, and the efforts necessary to bring down the head would, in all probability, tear the cicatrix where it involved the base of the bladder. There would then be a laceration of the bladder, and the same injury of all the parts interested, as in her first delivery; and the resistance was such from the callous state of the cicatrix, and the narrowness of the outlet, that there was no reason to believe that a living child could be brought through. After maturely considering these circumstances, we felt satisfied that the life of the child could not be saved; we, therefore, resolved to open the head, as by far the safest course for the mother. This was accordingly done in the usual manner, and the brains and a portion of the bones constituting the vault of the cranium taken away. The crotchet was then applied within the skull, and efforts made during the pains to extract. These proved for a long time unavailing, the bones always giving way when much force was applied. I at last got a firm hold, by introducing the instrument into the foramen magnum. It was kept there steadily, and exertions made during each pain, for nearly an hour, when at length the head moved a little; from this time the progress was regular until the head was delivered. The

body followed soon after without much difficulty; and in a few minutes more the placenta, which appeared to have been separated at the time the body was delivered. A soft warm cloth was then applied to the external parts, and an anodyne exhibited, and the strictest rest enjoined; and after waiting for a reasonable time for fear of hæmorrhage, we took our leave. She was again visited in the course of the day, and appeared to be doing as well as could have been expected. She was, however, troubled with incontinence of urine, which caused considerable irritation to the lacerated labium. On the following day, she suffered so much from the constant discharge of her urine, that I was induced to make an examination, to ascertain the condition of the different parts. I found that the upper part of the cicatrix, behind the symphysis of the pubis had given way, and that there was an opening in the bladder immediately behind the urethra, large enough to admit the point of the finger. To remedy this as far as practicable, I introduced a gum elastic catheter into the bladder, and secured it in its situation; I placed a soft sponge at its extremity, to receive any urine that might pass. A pledget of charpe, covered with simple cerate, was then applied to the opening in the bladder, and supported in its place by additional pledgets in the vagina, to prevent, if possible, the further discharge of urine. These dressings were renewed several times every day, and particular pains taken to keep the external parts clean. By this simple course, the opening in the bladder closed; she began to pass her water in the natural way; her general health daily improved; the injury done to the labium and vagina became repaired; and in three weeks she was about the house in comparative comfort. From the time of her first delivery to her second labour, she had great difficulty in walking, owing to the unnatural state of the parts about the external organs of generation; but since her recovery, she no longer experiences any inconvenience. For some time after the parts became healed, the cicatrix had not shown but a slight tendency to contract. How it has been since, I am not able to tell. It is, however, quite certain, that the passage might be dilated gradually to

its natural size, should she again become pregnant, and that a small child might be born without manual assistance. The outlet of the pelvis is so narrow, that it is not probable a living child could pass, if of large size.

This case affords a pretty strong evidence of the incorrectness of our most generally received theory of generation. From the smallness of the opening in the cicatrix, and its position behind the symphysis of the pubis, it is impossible that any part of the semen could have touched even the external part of the uterus at the time of its projection. Only a very small portion could have got through to the upper part of the vagina, and even that, from the form and situation of the narrow passage, could not have passed through with any force. The influence of this semen must, therefore, have in some other way been communicated to the ovaria from the vagina. How that might have taken place, we do not know. There may be a way not yet discovered, by which some part of the semen might have been directly conveyed to the ovary; or a peculiar impression made by it, might, perhaps, have been transmitted, and been sufficient to have vivified the germ. This may be all that is required to cause conception in the human species, for aught we know to the contrary. Impregnation has taken place in many instances, where the passage leading to the uterus has been so small as scarcely to admit a goose quill; and in other cases, the vagina is said to have been altogether closed by a membrane; so that there is reason to believe that conception is the result of a peculiar sensation or impression, produced by the semen during the venereal orgasm upon the external organs.

REVIEW.

ART. I. *Treatise on the Theory and Practice of Physic.* By GEORGE GREGORY, M. D. with notes and additions, adapted to the practice of the United States. By NATHANIEL POTTER, M. D. Professor of the Practice of Physic in the University of Maryland. And S. COLHOUN, M. D. &c. 2 vols. 8vo. Phila. 1826.

IF we estimate the responsibilities of authors, according to the influence their writings are calculated to have upon human happiness, there can be no question that, next to those who write for the moral improvement of the world, none are more answerable for correctness of fact and doctrine, than those which are strictly medical; and consequently, there are none whose writings are more legitimate objects of criticism. This remark, though applicable to every class of medical writers, is especially so to those who come before the public, with works professing to give a systematic view of the principles and practice of physic.

Doctor Gregory, in the work before us, has clearly proved himself adequate to the responsible labour in which he engaged. Unlike his popular predecessor, Dr. Thomas, he has evidently thought for himself; and instead of jumbling together the theories and practice of innumerable authors, has given an able, and, with a few exceptions, an exceedingly well digested account of the most approved pathological and therapeutic principles of the day.

The first peculiarity observed on looking into his work is, his method of arranging diseases. It is well known, that since the time of Plater, and particularly of Sydenham, medical philosophers have been at no little pains to generalize dis-

eases, according to the principles of classification adopted by the cultivators of natural history. The amount of labour which has been performed in works of this kind is immense. Sauvages, who is justly reputed the father of this department of science, in his Nosologia Methodica, published in 1763, displayed a force of genius, and extent of erudition rarely equalled. The classification of this illustrious physician, though admirable as a first attempt of the kind, is generally acknowledged to be defective and inapplicable to practice. In fact, it is rather to be regarded as a specimen of extraordinary industry and learning, than as a work serving to promote the advancement of medical science. Struck with the success which attended the labours of Sauvages, and prompted by a laudable ambition to improve this new branch of medicine, Linnæus, Vogel, Cullen, Selle, Macbride, Sagar, Plouquet, Darwin, Crichton, Pinel, Parr, Swediaur, Young, and Good, successively presented to the public systems of nosology, each pretending to greater simplicity, and stricter conformity to the natural method of classification.

With regard to the merits of these several systems, we have only to remark that, however ingenious their construction, no one of them is free from gross and palpable imperfections. Nor is it at all surprising that this should be the case. The objects of nature can never be correctly classified, until their discriminating characters are clearly ascertained. Now with respect to many diseases, we have yet to learn in what they essentially differ: their diagnosis is by no means well settled; and as regards their etiology and symptomatology, they are too various and equivocal to be made the ground-work of scientific arrangement. Sydenham, in contemplating the idea of classifying diseases, does not appear to have considered pathology in his day, as sufficiently improved to warrant such an undertaking. Sauvages, his contemporary, thought otherwise; and accordingly engaged in the "herculean labour," of laying the foundations of systematic nosology. His attempt, viewed in its results, was, undoubtedly, premature; and it may even be questioned, whether it had not a tendency to retard the progress of pathology, by cherishing a passion

for generalization, and consequently, withdrawing the attention from those analytical researches, which require to be completed before diseases can be definitely and scientifically systematized. That such has, in a degree, been the consequence, we think is obvious, from the fact, that since anatomy and physiology have been studied with a direct application to pathology, less attention has been given to the cultivation of nosology. It is true Dr. Good, and one or two other nosologists, have founded their respective systems upon an anatomical and physiological basis; and it must be confessed, that in doing so, they have made a nearer approach to a natural and unexceptionable classification of diseases, than any which preceded them. Still their systems are far from being perfect; and a tenacious adherence to them, must, we think, have the same prejudicial effect upon the interests of medicine, which we have supposed was produced by the Nosologia Methodica of Sauvages.

Views similar to these, probably induced Dr. Gregory, in planning his treatise on the theory and practice of physic, to deviate from the method pursued by nosologists, of arranging diseases into orders, genera, species, and varieties. His plan is more simple, and better suited to exhibit in detail the doctrines of pathology, and the rules of practice. He arranges diseases into two great divisions; viz. ACUTE and CHRONIC—a division first made by the Greeks, and approved by Celsus.

"The ancients," says Baglivi, "called those diseases acute, which being seated chiefly in, and attended with a rapid ebullition of the fluids, run their course quickly. On the other hand, they call such diseases chronic, as proceed from a vitiated condition of the solids of the body, or from preternatural grossness of the fluids, on which account they either move very slowly towards concoction, or else never reach it."

Though the division of diseases into acute and chronic is not free from objections, it is evidently well adapted to practical purposes. The following is the view taken by Dr. Gregory, of the general characters of these two kinds of diseases.

"The general character of chronic diseases may be viewed as the reverse of that which distinguishes diseases of an acute kind. Throughout the latter a considerable similarity of pathology will have been observed to prevail. There is a remarkable uniformity also in their symptoms and periods. They run their course in a short time—often in a defined time. In all of them may be traced a disposition to terminate in the recovery of health. Medicine exerts over the greater number of them a very obvious power; and the principles of their treatment may, in most instances, be considered as tolerably well ascertained.

"Chronic diseases, on the other hand, are very tedious: some of them may even be present in one shape or another during the whole course of life. In their progress they are very irregular. The protæan forms which they assume not only perplex the practitioner, but oppose, at the same time, the most serious obstacles to their accurate description. Though not commonly, or necessarily, accompanied by fever, yet feverish symptoms may arise in all of them, at any period of their course. Much obscurity pervades their pathology. The reasonings concerning some of them do not readily assimilate with the views entertained of other disorders. Lastly, the principles of treatment in chronic diseases are neither uniform nor well understood. In many instances they are wholly unknown; but were they even better ascertained, it is doubtful how far the physician could avail himself of such knowledge. In the cure of chronic diseases, indeed, neither fortune nor art avail him much. It is seldom that he observes in them any disposition to terminate spontaneously in the recovery of health; and they are unquestionably much less under the control of medicines than acute diseases."—Vol. II. pp. 5, 6.

Dr. Gregory divides acute diseases into four classes, viz. 1. Fevers. 2. Exanthemata, or eruptive fevers. 3. Phlegmasiæ, or inflammatory diseases. 4. Hemorrhages.

Chronic diseases he divides into five classes, viz. 1. Chronic diseases of the encephalon. 2. Chronic diseases of the thorax. 3. Chronic diseases of the chylopoietic viscera. 4. Chronic diseases of the urinary and uterine systems. 5. Chronic constitutional diseases.

In treating of the diseases belonging to these several classes, the author, instead of grouping them into orders and genera, has made them subjects of distinct chapters. His plan is, if the expression be allowable, monographic; while at the same time, he strictly adheres to his classic arrangement. Each disease is particularly considered in its character, symptomatology, etiology, diagnosis, prognosis, terminations, treatment, and relations to other disorders.

It is worthy of remark, that the general scheme of the author, though of high antiquity, is, in its details, made to quadrate with the present improved mode of studying the nature and seats of diseases. Thus his division of acute diseases embraces those complaints which are chiefly manifested in the vascular system, corresponding, in the main, with Dr. Cullen's class Pyrexia, and Dr. Good's Hematica. As chronic diseases vary in their seats, no less than in their causes and pathology, he properly assembles all those into one class which affect a particular organ, or system of organs, beginning with those of the brain and nervous system, and proceeding to those of the chest, chylopoietic viscera, &c.; closing with those which cannot be referred to any particular organ, but which he regards as constitutional, depending upon a depraved condition of the whole system.

In the first chapter on febrile diseases, the author discusses the general doctrine of fever. After stating the importance of the subject, and enumerating the phenomena, leading divisions, and predisposing and occasional causes of this class of diseases, he proceeds to some general observations on the nature of fever. In what particular state of the fluids or solids of the body, he asks, does it consist? The theories of Hippocrates, Stahl, Boerhaave, Hoffman, Cullen, and Philip, are then severally noticed. He thinks the most rational views of the intimate nature of fever, are those of Hoffman, who believed that fever consisted *primarily in diminished energy of the nervous system.* But he remarks,—

"All of these theories are open to many and strong objections. An insuperable difficulty indeed seems to hang over the pathology of fever, but it is fortunately of little moment. No theory of the proximate cause of fever which has yet appeared, has contributed in any material degree to improve the treatment; though several of them, especially the Hippocratic, have had the effect of misleading and confusing the practitioner. The phenomena of fever give evidence of diminished energy of the brain, with increased action of the heart and arterial system; and the difficulty in the pathology of fever consists in showing, in what manner these disturbances of function are connected with each other. The older pathologists supposed it was brought about by the *vis medicatrix naturæ*, for which in modern times we have substituted the principle of *re-action;* but the pre-

cise mode in which this re-action of the heart and arteries is effected appears to be altogether inscrutable.

"To the diminished energy of the nervous system we ascribe the languor, lassitude, loss of appetite, general uneasiness and pain in the back, which mark the invasion of fever. The functions of the brain not being as yet thoroughly understood, it is doubtful whether or not we are authorised in attributing to the same source the diminished and depraved secretion which occurs in fever; but it is highly probable that the phenomenon is in some measure connected with it. The same thing may be said of the increased heat which attends fever, the physiology of animal heat being, like that of secretion, involved in much obscurity. It would appear, however, that this is a mixed function, in which the brain and heart are both essentially concerned."—Vol. I. p. 21, 22.

The phlegmasial theories of fever, advanced by Clutterbuck and Broussais, are not noticed by Dr. Gregory. The reason of this we are left to conjecture. As the author is decidedly of opinion that fever, whatever be its proximate cause, is essentially idiopathic, or, in other words, a disease of the whole system, he probably deemed it unnecessary, in an elementary work, to examine doctrines so opposite to his own, and those generally approved by his countrymen. Dr. Colhoun, in the American edition, has briefly stated, in a note, the theories in question; and also those of Bianchi, Swalve and Rhan, who, like Clutterbuck and Broussais, contend that the source of fever is always an inflammation of some particular organ or organs. "As no facts are adduced by the advocates of these doctrines, to show that the local affections preceded the fever," says Dr. C. "which they should do, if they were its causes, and as there are many fevers in which there is no inflammation nor affection of any local kind whatever, these opinions necessarily fall to the ground."

The method pursued by Dr. Gregory, in treating of particular fevers is, we think, extremely objectionable; indeed, we can conceive of no arrangement more likely to render the study of fevers difficult and unsatisfactory. Of the justness of this remark, the reader will judge after noting the titles and order of his chapters as here quoted. Chap. 2. Varieties and symptoms of continued fever. Chap. 3. Causes of con-

tinued fever. Chap. 4. Treatment of continued fever. Chap. 5. Of the plague. Chap. 6. Intermittent and remittent fevers. Chap. 7. Of the yellow-fever.

In this arrangement it will be observed, that the author commences with continued fevers, such as the synocha, synochus, and typhus of Dr Cullen, instead of beginning as most systematics do, with intermittent and remittent fevers. In medicine, as in the exact sciences, subjects the least difficult of analysis, should always be studied before those which are more abstract and complex. It is in this way only, that we are enabled to advance with facility to the higher departments of knowledge. Now surely, the author would have been more successful in communicating to the student a clear and precise idea of the nature and peculiarities of fever, had he first treated of intermittents and remittents, and subsequently described the various, complicated, and changing forms of continued fevers. This, we cannot but think, Dr. Gregory will, on reflection, himself concede; since in the commencement of his chapter on the varieties and symptoms of continued fevers, he tells us, that "the views of physicians, with regard to continued fevers, have undergone a number of very remarkable changes, to which nothing more essentially contributed than the *infinite diversity of symptoms by which they are characterised.* Nosologists have been at great pains to note minutely these different symptoms, and have founded upon them their divisions of continued fever. Boerhaave has three, Linnæus four, Sauvages five, and Macbride five and twenty species of continued fever." In preparing future editions of his work, it is hoped, the able author will improve his arrangement of fevers in the way we have suggested.

On the subject of febrile contagion, Dr. Gregory entertains opinions similar to those of most of his countrymen. He says that,—

"Much confusion has been introduced into the subject of contagion, by the employment of the term *infection*, and by the different acceptations in which contagion and infection have been taken. This has been increased by the want of a proper distinction between common contagion and specific contagion. Diseases which cannot be produced in any other way

than by contagion, are said to have their origin in *specific contagion.* Of this kind are small-pox, cow-pox, measles, the plague, hydrophobia, and syphilis. Diseases which, occasionally produced by contagion, are yet sometimes owing to the operation of other causes, are said to arise from *common contagion.* Of this kind are catarrh, cynanche parotidæa, erysipelas, ophthalmia, typhus, and scarlatina. The laws of common and specific contagion are in many respects similar, but they have also their points of difference.

"By a few, and happily a very few, it has been contended, that the notion of a contagious origin of typhus fever is altogether unwarranted; but the views of these *anti-contagionists* are so completely at variance with the generally received opinions of medical men, and so irreconcilable with facts obvious to all mankind, that any formal refutation of them is unnecessary. On the other hand, there have been, and there continue to be, physicians who believe in the *exclusive* origin of typhus from contagion, who maintain that no disease can propagate itself by contagion which had not its own origin in contagion; in other words, who deny that common continued fever, under any, the most adverse circumstances, can ever spread by contagion. This opinion involves the difficult, but for the most part idle question, how contagious fevers ever originated; but setting this aside, it may fairly be argued that it is neither borne out by observation nor by reasoning. There is nothing improbable in the supposition, that what originated in cold may be afterwards propagated by contagion. It violates no established law of the animal economy. Experience on the other hand appears to favour it; and it may therefore be laid down as an important practical principle, that fever, which originated in the first instance from *common* causes, may under certain circumstances, either of local situation or constitution of body, spread by contagion. What those particular circumstances are, which thus concur to favour the developement of febrile contagion, may be anticipated from remarks already offered. The principal of them are, crowded and ill-ventilated apartments, want of cleanliness and comfort, and previous weakness of the affected individual, whether owing to excessive fatigue, or an unwholesome or scanty diet."—pp. 54, 55.

We copy the preceding passages, in order to caution the American student against an inconsiderate adoption of the doctrine they inculcate; a doctrine, so far as it relates to idiopathic fevers, as typhus, plague, and yellow-fever, rejected by the great body of American physicians; a doctrine, founded in prejudice, and fraught, in its practical application, with serious mischiefs. As our limits do not allow us to examine the particular views of the author, in regard to contagion, we must content ourselves with observing that Dr. Potter has, in a note, page 143, judiciously corrected his

erroneous statements. Before forming his opinions on this subject, the student should consult the writings of Rush, Miller, Caldwell, Pascalis, Potter, and the various medical journals of our country. In these, he will find ample and demonstrative proofs of the unconditional non-contagiousness of fevers.

In detailing the treatment of febrile diseases, Dr. Gregory displays much soundness of judgment, and correctness of remark. The rules laid down by him, for the use of stimulants in the low, or collapsed state of continued fever, will be approved by every physician, whose opinions are founded upon accurate clinical observation. Guided by his precepts, the student will happily avoid the deplorable consequences, which frequently follow the use of stimuli, when employed to the extent advocated by some American physicians, and especially by the author of the "Observations on fever," appended to Dr. Thomas' Practice of Physic.

"The great weakness," says Dr. G. "which prevails in fever, naturally led to the employment of cordial and tonic medicines, more particularly wine, ether, camphor, musk, bark, and aromatics; but it is now generally acknowledged, that the indiscriminate use of stimulant remedies in fever is highly pernicious; that they have a tendency to aggravate many of those local determinations, from which danger is chiefly to be apprehended; and, therefore, that their employment is to be regulated by circumstances, no less than that of blood-letting. The period of the disease, the particular situation in which it appears, its exciting cause, the age, constitution, and former habits of the patient, are of course to be taken into account; but we are chiefly to be guided by the *character of the symptoms*, and the *effects of the remedies*.

"In the state of true collapse, marked by cold and clammy sweats, a moist, brown, and loaded state of the tongue, stimulants, especially wine or brandy, are not only beneficial but absolutely necessary. Such symptoms are clearly indicative of a failure of the powers of life, and unless stimulants are duly supplied in quantities proportioned to the exigencies of the case, the patient rapidly sinks.

"Those cases of typhus which are accompanied by petechiæ, or the large livid blotches called vibices, in short, by what we have denominated the symptoms of putrescency, are benefitted by the steady and moderate exhibition of wine, bark, and aromatics.

"There is a third class of symptoms which has been supposed to indicate the propriety of a similar plan of treatment; I mean those which denote

irregularity in the action of the nervous power, such for instance as subsultus tendinum, picking of the bed-clothes, and tremulous tongue. These are distinctly symptomatic of cerebral irritation, of a state which is indeed sometimes relieved, but not unfrequently aggravated, by wine and bark. If these symptoms are present, along with a parched tongue, a hot and dry skin, and any degree of *sharpness* of the pulse, wine even in small quantity is generally hurtful. It is a state which may often be better combated by local bleeding, blistering, and laxatives. Wine is indeed at most times a doubtful remedy in fever, which should never be persevered in, unless the signs of improvement are very unequivocal.

"2. The effects of all stimulant remedies are to be carefully watched. Even when most essentially required, as in the lowest state of collapse, they will sometimes occasion a degree of excitement, from which danger may be apprehended. If the tongue under their exhibition becomes dry, and delirium increases, they should be immediately diminished, or altogether withdrawn. If the patient is upon the whole improving, this should satisfy us. Any attempt to accelerate his recovery by increasing the quantity of wine, will only risk his safety."—pp. 81—87.

The class of exanthemata is treated by the author in a very satisfactory manner. He divides this part of his work into six chapters. In the first he points out the relation which the exanthemata bear to simple fevers, to one another, to other diseases arising from morbid poisons, and to cutaneous diseases. Having given a general idea of these objects of inquiry, he directs his attention in the five following chapters, to the medical history of the small-pox, chicken-pox, cow-pox, modified small-pox, measles, scarlet fever, and the minor exanthemata, comprising herpes, urticaria, lichen, roseola, pompholyx, and frambæsia.

It is remarked by Dr. Gregory, in speaking of small-pox, that "the general introduction of the practice of inoculation was brought about in 1722, by the accurate observation, and spirited efforts of Lady Mary Wortley Montague." That this distinguished personage was instrumental in introducing the practice of inoculation into England, is abundantly attested by the annals of her time; but it would seem from the statement given us in a note by Dr. Colhoun, that she was by no means the sole or principal agent engaged in establishing the practice in question.

"It is not generally known," says Dr. C. "that for the successful and ameliorating influence of inoculation for the small-pox, we are more indebted to one of the then Colonies than to Great Britain herself. We feel a pleasure and a pride in bestowing the due meed of praise upon the state of Massachusetts. Lady Montague's elder daughter had been inoculated at Pera in April, 1717, by Surgeon Maitland, surgeon to the embassy, and by the same hand, her infant daughter aged three months, was inoculated in London, in April, 1721. The elder was the first European inoculated for the small-pox, and the younger the first inoculated in Europe. In April, 1721, inoculation for the small-pox was introduced into Boston by the Rev. C. Mather, aided by Dr. Boylston. This rev. gentleman received the suggestion from some ephemeral account he had read of inoculation in the East. He was destined to pass the ordeal of all the ignorance, prejudices, and superstitions of the times, not only of the populace, but the faculty of physick, who were, with the exception of Dr. Boylston, enlisted against the project. A very protracted public controversy ensued, in which the clergy generally united against the physicians, and finally, so signally triumphed, that inoculation spread through most of the then Colonies, and the practice was adopted without opposition long before the dissensions ceased in England. The success of the Rev. Mr. Mather, and his able and spirited coadjutor, Dr. Boylston, contributed very essentially to settle public opinion in Great Britain, and to promote the progress of inoculation in other parts of Europe."—p. 178.

In adverting to the merits of the controversy, respecting the identity of small-pox and chicken-pox, Dr. Gregory, though unwilling to commit himself, is inclined to side with those who consider them distinct diseases. He observes,—

"It has been rendered highly probable that the genuine varicella is not communicable by inoculation; but it has at the same time been shown, that many cases of *supposed* varicella do produce a disease by inoculation, which is not chicken-pox, but small-pox. Reasoning from these data, some modern authors have retained the notion of the specific disease varicella, but given it new characters; while others have revived a doctrine which prevailed very generally in former times, and was distinctly avowed by Sauvages; viz. that chicken-pox and small-pox originate in one and the same contagion. In support of the latter opinion, many ingenious arguments have been brought forward in a late essay, which has certainly thrown much light upon the history of those eruptive diseases, connected in their origin or symptoms with variola. As this hypothesis, however, is still involved in many difficulties, it will be proper here to adopt the former notion, to consider varicella as a disease altogether distinct from variola and to state its distinguishing characters."—p. 198.

The observations of Dr. Gregory on the varioloid disease, or modified small-pox, are few, but comprehensive. It is now placed beyond all question, that vaccination, however perfect in appearance, does not in every case, secure the system against an attack of small-pox. In fact, as Dr. G. well remarks, every year's experience serves more and more to show, that a certain proportion of those who have undergone vaccination, will take small-pox at a subsequent period of their lives. The author gives a rapid sketch of the circumstances under which this occurs, the causes to which it may be ascribed, and the characters of the diseases so produced. He states that,—

"The disease has been by some ascribed to incomplete vaccination, and the notion is probably to a certain degree correct; for though it has been observed in a few cases where the progress of the vaccine vesicle was to all appearance regular, yet it has rarely occurred to me to witness it, and hardly ever in a severe form, where the cicatrix was perfect. Deterioration of the vaccine virus from successive inoculations has been brought forward by other pathologists, as calculated to explain the occurrence of small-pox after vaccination. This opinion, however, is unsupported by any arguments. As little foundation is there for the hypothesis of a *spurious* cow-pox, once formed to explain some of the anomalies which this disease presents. Taking all the evidence that has been afforded us respecting small-pox after vaccination, it appears that we must seek for its cause partly in the imperfect saturation of the system with the vaccine influence, and partly in that law of the animal economy which regulates the susceptibility of variolous contagion. Natural small-pox in its severest form does not always afford protection from a subsequent attack of the disease. To that *peculiarity of constitution* which favours secondary small-pox, we must be content to refer those cases in which small-pox occurs subsequent to *perfect* vaccination."—p. 208.

In no part of his work, has the author displayed a more perfect acquaintance with the present improved state of pathological science, than in his chapters relating to the phlegmasiæ. In the first chapter he describes the symptoms, terminations, and remote causes of inflammation; in the second, he treats of its varieties, proximate cause, and the general system of treatment. The principal sources of the varieties of phlogosis, he says, are, 1. The situation and function of

the organ inflamed. 2. The structure of the part inflamed. 3. The exciting cause. Of the sources of distinctions among inflammations, the most important is undoubtedly to be found in the structure of the part affected. Thus the cellular, serous, mucous, dermoid and fibrous textures, produce the five varieties of inflammation distinguished by the terms phlegmonous, serous, mucous, erysipelatous, and rheumatic. The distinctive characters of these varieties, are pointed out by the author with his usual perspicuity. In noticing that important and well established fact, that inflammation, in by far the larger proportion of cases, is confined to one texture, and that it generally extends along that one, without affecting other contiguous textures, Dr. G. asserts, that Dr. Carmichael Smyth, has undoubtedly the merit of being the first who thought deeply, and wrote expressly on the subject.

Though we have already occupied much of the space originally allotted to this article, we cannot but avail ourselves of this occasion to enrich our pages with a part of the author's interesting observations on chronic inflammation. His chapter on this subject commences with the following remarks.

"Chronic inflammation is a term frequently made use of; but I am not acquainted with any work in our language, which may serve to point out the pathological considerations which it involves. On this account, though the subject is perhaps too obscure for investigation in an elementary work, I have thought it advisable to offer a few remarks concerning it; rather, indeed, with the idea of attracting the attention, than of satisfying the inquiries of the student."—p. 280.

Chronic inflammation attacks almost every variety of structure. It occurs in the brain, lungs, liver, spleen, and kidneys. It affects the serous, mucous, and fibrous membranes, and also the substance of muscles. Of all the textures, the skin is the least liable to chronic inflammation, unless, as the author observes, we place in this class, lepra, herpes, and other varieties of chronic cutaneous diseases.

"One of the circumstances most deserving of attention in the pathology of chronic inflammation is, that it is sometimes a primary, and at other times a secondary affection. By this latter term, it is intended to imply, that it succeeds acute inflammation; and this is the most common form in which

it appears. It is thus that it occurs in gleet and dysentery. But at other times, chronic inflammation is not preceded by any marks of *active* inflammation. It begins almost imperceptibly, and its advances are slow, often exceedingly insidious, being unaccompanied by any symptoms which could betray, even to the experienced practitioner, the existence of such a disease. Nowhere is this better exemplified than in some forms of chronic peritonæal inflammation; but the same thing has been observed also in cases of chronic inflammation of the membranes of the brain, and even of the heart itself. In these instances, not only are there wanting all local symptoms of inflammatory action, but there are not even any constitutional symptoms; at least none of sufficient importance to attract attention. This, however, it must be confessed, is comparatively a rare occurrence; and it is much more usual for chronic inflammation, both primary and secondary, to exhibit local and constitutional symptoms, less in degree, but the same in kind, with those which accompany acute inflammation."—p. 281.

The etiology of *primary* chronic inflammation is a subject of great obscurity. Cold and mechanical irritation in some instances, appear to induce this kind of phlogosis. A strumous constitution has been thought favourable to its occurrence; but it frequently takes place where this cannot be supposed to have any agency in its production.

"The effects of chronic inflammation, or, more correctly, the local appearances presented during the state of chronic inflammation, vary with the texture of the part affected. A simple thickening of structure is a common appearance, both in serous, mucous, and cellular membranes. Sometimes the thickening assumes the form which has been called *tuberculated accretion.* At other times the part inflamed is converted into cartilage and bone. Instances of ossific deposition taking place in consequence of chronic inflammation, occur in chronic laryngitis, chronic pleurisy, and chronic pericarditis. A further effect of chronic inflammation (but one confined to serous membrane) is the extensive union of opposite surfaces. *Scirrhus* is generally accounted the effect of chronic inflammation in a glandular organ.

"The origin of *tumours* in different structures is a subject that has excited much attention among pathologists. In many cases it is presumed that their growth is referable to the same action of vessels by which all parts of the body are formed; but in other cases, there is reason to believe that they may have had their origin in a state of chronic inflammation of vessels. Closely allied to tumours are *tubercles;* but the views which are entertained by pathologists of the origin and progress of tubercle, will come better under discussion hereafter, when treating of pulmonary consumption.

"The last effect of chronic inflammation which I shall notice is *suppuration*, and it is one of those which we have most frequent occasion to witness in practice. The fact of the formation of purulent matter in cysts and other structures, without any evidences of previous inflammation, was well known to John Hunter, who had particular views of his own regarding it. But they are very unsatisfactory; and until further light is thrown upon the subject, it may not be improper to consider these collections of matter as the result of chronic inflammation."—pp. 283, 284.

The views of the author with respect to the therapeutic management of chronic inflammation, are stated with brevity and clearness.

"The treatment of chronic inflammation is very little understood. It is often said, that parts which have been much weakened, especially by large bleeding during the acute stage, are liable to fall afterwards into the state of chronic inflammation. I believe, however, that the mark is not of general application, and that this form of disease is oftener attributable to a neglect of those vigorous measures which would have cut short the acute stage of inflammation at its commencement. Chronic inflammation is almost as much out of the control of medicine as acute inflammation is under it. Nature sometimes works a cure; but in many cases, more particularly of primary chronic inflammation, the prognosis is very unfavourable.

"The general system of treatment must depend upon the state of the constitution. Four plans of treatment have been advised, and each has been found serviceable under different circumstances.

"1. Where fever is present, blood-letting, purging, and saline medicines, with a low diet, are to be recommended.

"2. Where the pulse is feeble, and there is a decided loss of tone in the system, myrrh, benzoin, the balsam of copaiba, steel, and bark, are unquestionably useful.

"3. Where the disease is purely local, it is best treated by leeches, blisters, and issues, upon the principle of counter-irritation.

"4. Where these means fail, an *alternative* plan of treatment may be resorted to. This is done under the idea of giving a new action to the vessels. Upon this principle, mercury is employed in the treatment of chronic hepatitis, alkalies in the scrofulous inflammation of absorbent glands, and sarsparilla and guiacum in chronic rheumatism."—pp. 285, 286.

A small part only of Dr. Gregory's work is devoted to the class of hemorrhages. Believing that these discharges are seldom met with in an idiopathic form, he asserts, that it is only by a nosological refinement, that they can be considered

in the light of a distinct order of diseases. He observes, that the rupture of a blood-vessel is not necessarily connected with a train of other symptoms, and is, therefore, rather an accident, or a *symptom*, than a state of disease. "While engaged in the investigation of the phlegmasiæ," he continues, "we were content to refer the phenomena to the presence of *inflammation*. In the class of hemorrhages, we must always look to something beyond, and endeavour to determine upon what ulterior cause the rupture of the vessel depends.

The author divides hemorrhages into general and local. As examples of the former kind, he mentions scurvy and *hæmorrhœa petechialis*. Local hemorrhages he arranges according as they occur in one or other of the great cavities or divisions of the body, namely, the head, chest, and abdomen. He observes—

"It is obvious, that in estimating the circumstances which may lead to the accidental rupture of a vessel in an internal part, there are three which chiefly merit attention. The first of these is the quantity of blood in the body; the second is the force of the heart's action (these two constituting the impetus, or *momentum* of the blood:) and the third is the strength of the coats of the containing vessel, depending principally on the *original* constitution of the body. By one or other of these considerations, we may explain the manner in which different circumstances act as the predisposing or occasional causes of hæmorrhagy, and the *modus operandi* of the remedies which are resorted to for its relief or removal."—p. 513.

Dr. Gregory is disposed to dissent from the opinion entertained by some pathologists, that mere *laxity* of the coats of vessels, independent of actual rupture, is sufficient to cause the effusion of blood. He thinks it probable, that in certain relaxed conditions of a membrane, the colouring particles of the blood may exude along with the secretions of the part; but how far this corresponds with genuine hemorrhage, he says is questionable. The facts and considerations urged by Bichat, Laennec, and some other pathologists, are, we think, sufficiently strong to warrant us in receiving it as a truth, that hemorrhages do occur without an actual rupture of blood-vessels. Bichat has distinctly shown that they may take place from the *exhalents* of the mucous and serous tissues.

In classing hemorrhages, he distinguishes them, 1, Into those that come from exhalation; 2, Into those that are produced by rupture.* Laennec says, "in some cases there is no doubt that a *very copious exhalation* of blood may take place spontaneously, without any solution of continuity or external violence. I do not allude to those effusions which, as we have already seen, sometimes attend the function of blood-vessels in the false membranes, or which confer on certain other effusions a sanguineous tint merely, but to a primary and idiopathic effusion of blood analogous to the hemorrhages, active or passive, of other organs.†"

Having now taken a general view Dr. Gregory's division of acute diseases, it would be interesting to follow him through his observations on chronic disorders; but our limits require that we should bring this article to a close. Though we have made a few objections to the author's arrangement and doctrines, we think, upon the whole, he has presented, in a neat medical style, more lucid and correct views of pathology, than are to be found in any strictly elementary work on the practice of physic.

Before taking leave of the work before us, we cannot but observe to the gentlemen who assumed the responsibility of enlarging and republishing it for the benefit of American students, that, however valuable their notes and additions, the manner in which many of them are introduced, is not only injudicious, but positively censurable. We think, and we are not alone in the opinion, that they have taken an unwarrantable liberty in breaking the continuity of the text, often in the middle of sentences, for the purpose of inserting what they conceived necessary to render the work better adapted to American practice.

---

* General Anatomy. Exhalent system.

† Treatise on Diseases of the chest, p. 155. Am. edit.

ART. II. *De la Lithotritie, ou Broiement de la pierre dans la vessie.* Par le DOCTEUR CIVIALE, avec cinq planches. 8vo. pp. 254. a Paris, 1827.

SURGERY has, perhaps, approached nearer the exact sciences than any other department of medicine; and the improvements which it has received in modern days, must be justly attributed to observations derived from the laws of physiology and pathology, which our present state of knowledge has enabled us to accumulate. The rage for *cutting* has indeed, in no small degree, retarded the anticipated progress of this science. This, however, we are convinced is rapidly passing away, at least among the more intelligent and enlightened members of the profession. Mr. Lawrence in his introductory of this year, has stated, "that twenty-five years ago there were three times as many operations as now performed at St. Bartholomew's Hospital." The work now under analysis, introduces an improvement which will still further diminish the field upon which so many ignorant pretenders have presented their claims to celebrity and patronage. But further than this, it has the high merit of not only preventing the hazard of life, but of completing a cure without leaving any of the inconveniences, to say the least of them, attendant upon the method commonly employed. However perfect our knowledge of the anatomy of the parts concerned in lithotomy, deaths have occurred, in which the most competent skill has been employed. It must not be thought, however, that M. Civiale's method is applicable to all cases; he candidly confesses it is not, and points out in what particular conditions the common plan must be pursued. Yet to many it is well adapted, and to which *cystotomie* was heretofore the only successful remedy. The claims advanced by the author were received with great caution in France, and it was not until four years after his application to the Academy, at which time a commission was employed, that it consented to act, and not until he had been successful in private practice. The profession at large, do not easily abandon old habits, and they must be well convinced that speculation and innovation have not only the

name of improvement, but the sanction of experience, before they can be adopted. The unceasing vexations which poverty and distrust threw into M. Civiale's way, could only meet its reward in the invidious competitions of unsuccessful rivals, and the candour and approbation of the liberal and enlightened of his brethren. In what manner it will be received in this country, we are in no doubt, as regards the scientific part of our profession; but among those whose boast is in the number of limbs excised, and not in those that have been saved, we must not expect that it will be received with much favour. But their wishes, we are happy to say, do not interpret the views of the profession; and we hope, when opportunities occur suitable to this operation, it will receive a fair trial and impartial investigation. We shall now proceed to give an analysis of the work, adopting the language of the author, or condensing his matter as may appear most advantageous; illustrating by cases, the principal points he is anxious to prove.

His work is divided into the following heads. The formation of urinary calculi; action of the calculi upon the animal economy; history of the "Lithotritie;" the anatomy of the urethra; catheter and its use; the apparatus used; the manner of operating; its application, and objections to it; stricture of the urethra. It concludes with the report of the Academy of Royal Sciences.

Chapter I. *Formation of urinary calculi.* It is difficult to explain the mode in which urinary calculi are formed. This arises from the numerous elements which enter into the composition of the urine, and its variation in different indivials, temperaments, climates, &c. Calculi differ in their form, their hardness, their volume, and their number. Oxalate of lime is always round. Phosphate of lime and magnesia are less determined; uric acid and urate of ammonia, are in general ovoid and slightly flattened. When there are many calculi in the bladder, they present usually the same facettes. When a calculus fills the bladder, it serves as a mould for it, having a corresponding furrow on each side at the orifice of the ureters. Stones which are formed from the accidental introduction of

a foreign body, resemble more or less their nucleus. Lamotte speaks of one which was formed about a wire. The hardness of calculi depends not only on their composition, but their age. Their formation sometimes is very rapid. We have seen two cases where a calculus in the space of six months, acquired the size of an ordinary walnut. This indicates phosphate of lime, which is very friable, and which is susceptible of becoming larger than any others.

The urates and ammoniaco-magnesian phosphates do not in general increase after they have been in the bladder long. The oxalates form more slowly. The number of calculi are usually from one to two; though Boerhaave says, he has found three hundred. This appears very great, but is confirmed by the observations of our own times. M. Murat found six hundred and seventy-eight; and M. Beauchene three hundred. Fifty triangular calculi were taken from Buffon after death. Flurant says, he extracted twenty-four stones, of the size of a pigeon's egg. Our author has endeavoured to determine the number afflicted with calculi, in proportion to the population, but without success. More than half of those who have stone have not reached puberty. Between four and nine it is most frequent. The proportion of females to males is one in twenty. From researches made in England by Prout and Marcet, out of two thousand two hundred calculi, eighty-eight were taken from females. Their analysis also shows that the disease declares itself before puberty. It is most frequent in temperate climates. *Causes.* Sedentary life, too much animal food, urine long remaining in the bladder from stricture, introduction of foreign bodies. Siebold, in his preface to Langenbeck says, he extracted a calculus whose nucleus was a leaden bullet.

Chapter II. *Action of calculi upon the animal economy.* M. Civiale details the most prominent symptoms too well known to need repetition. The autopsy is interesting. The bladder shows sometimes a superficial inflammation, but more generally deep seated, and running into gangrene, often deep ulcerations. The muscular fibres of the bladder show themselves very

strongly; its walls are thickened, and its capacity diminished; in other cases, it is very much enlarged, and its sides thinner that natural, and without a trace of inflammation. The adjacent parts partake of the disease. The kidneys are always affected.

M. Civiale here relates many cases productive of different symptoms. It sometimes happens, that the bladder contracts so violently upon the stone, that its asperities force themselves into its tissue. In one patient after death, the stone had the appearance of a stalactite penetrating the bladder.

This chapter is closed by numerous cases, in which the constitution was so much affected, that all hopes of success were abandoned.

Chapter III. "*Of Lithotritie.*" M. Civiale, from the study of cystotomie, formed a project for relieving the sufferings of those afflicted with stone, by the employment of solvents. After several fruitless attempts, he abandoned it as impracticable. He then contrived two instruments; one to seize the stone and perforate it; the other to insulate the foreign body, and to preserve the bladder from injury. The difficulty of finding a mechanic, and the complexity of the instrument involving great expense, induced him to apply to the minister of the interior. The minister referred his petition to the Faculty of Medicine of Paris, who committed it to M. M. Percy and Chaussier, who did not think proper to report, and the minister accordingly did not accede to his request. He was thus obliged to abandon his project, which had already occupied him a year. In the beginning of 1819, he completed the first and most complicated of his instruments. Experiments were made on the dead body with the instrument in its most imperfect state. After contending with great difficulty in its construction, in 1820 he instituted a series of experiments on living animals These trials on the dead body and on living animals, suggested new improvements: in 1822 he further perfected his designs. Accordingly he experimented on different calculi placed in the dead body, to learn the duration of time it took to break them up. At the end of

1823 he completed his instrument, on which he had been employed five years.

In this year he made trial on the living subject with success. This made him anxious to bring it before the learned of his profession: but the circumstances which accompanied its first presentation to the Royal Academy did not permit him, as long as doubts existed of its successful application, unless these doubts were destroyed by numerous facts. In 1824, an occasion was afforded by some patients who consented publicly to a trial of his method, which was thought chimerical by a majority of practitioners. Three patients offered: the first was fixed for January 12th, 1824. The day before, he submitted his method to the Commission of the Royal Academy. They were witness of the operation, and reported it a discovery "glorious to French surgery, and honourable to its author."

Chapter IV. This relates to the *anatomy of the urethra*. This canal furnishes three divisions of importance: the first from the orifice to the bulb The orifice is the most narrow. The navicular fossa comes immediately after: its diameter is greatest. From this part to the bulb the canal is uniform. The second division is from the bulb to the prostate. With the exception of the orifice, the bulbous portion is the narrowest; the membranous has the largest calibre. Sometimes large calculi stop here, and acquire a prodigious size, as large as a pigeon's egg. The third part is enclosed by the prostate, and terminates at the orifice of the bladder. The study of this division is of great practical importance.

Chapter V. *Of "Catherisme" by the straight sound.* For a long time it was deemed impossible to enter the bladder with straight sounds, always giving the instrument a curve, adapted as it was thought, to that of the urethra. This doctrine had received the sanction of ages, and even in our time, the introduction of a straight sound was supposed impracticable. These sounds were found in the ruins of Herculaneum. Albucasis, their inventor, has given us a figure of them. Lieu-

taud, Santerelli, and Larzas, speak of them also, although they did not use them. M. Civiale gives the following direction for their introduction. "The surgeon places himself on the right side of the patient, or between his legs. He brings down the staff by a gentle motion parallel with the thighs, which should be slightly bent. The instrument held with the right hand is introduced, and passes easily to the symphysis pubis. Here you approach the bulb; depress the staff, and then direct the beak of the instrument a little higher, and you pass without difficulty the membranous portion, and reach the prostate. It is necessary to give the sound the direction just indicated, for otherwise, a false passage will be exposed, and the point of the instrument will pass into the hollow at the bulb, and pass the inferior walls of the urethra to the anus. This has happened. The remaining part of the operation requires great care. If the prostate is sound, you can turn your hand, and raise the point of the instrument. The sound penetrates the bladder without pain. When the prostate is diseased, this rule will not apply. It is like feeling in the dark. and demands great experience for a guide.

M. Civiale presses upon our consideration, the consequence of the unskilful introduction of the straight sound; viz. the engorgement of the prostate, which afterwards in old men, prevents its use. He further says, the use of the sound is not so easy as many imagine, from operating on the dead body.

Chapter VI. This treats of the instrument; a description of which, without the plate, would be useless.

Chapter VII. *Of the steps af the operation.* First—preparatory treatment. The general health is to be narrowly watched. Antiphlogistic means are to be adopted, and local remedies to diminish the sensibility of the urethra, and accustom the canal to the presence of a strange body by the repeated introduction of the sound. One sitting, for eight days, ten minutes at a time, will be generally sufficient. A sound of small dimensions is used, and gradually increased, until the canal is sufficiently dilated. Second—the operation. The patient is placed on a bed, with a cushion under the sacrum, that the stone may rest in the posterior part of the

bladder. The common sound is introduced, and warm water, or an emollient decoction, is injected according to the capacity of the bladder, which must remain until the patient has a desire to expel it: the sound is then withdrawn, and the instrument immediately passed into the bladder. Commonly the stone is felt; if otherwise the branches of the litholabe are opened, and it is seized. The other parts of the operation are described with reference to the instrument. When it is finished, a bath, and some hours of rest, and a mild regimen; but diet is rarely necessary. The day after, the patient is as well as the day before the operation.

CHAPTER VIII. *The application of the "Lithotritie."* M. Civiale arranges patients into three classes. *First.* Those under the most favourable circumstances, as where the calculi are small, the bladder little affected, the urethra, prostate, and general health sound. We shall cite cases under all these divisions. "M. Fechen, aged forty years, experienced since six months past, pains in the pelvic region, which sensibly increased, notwithstanding the means employed for their relief. This state of suffering, which the least fatigue augmented, was accompanied by an observable change in his urine. He rapidly lost his strength and flesh, and he was obliged to give up his business. He consulted me. I thought he had stone, which was confirmed by examination. The calculus appeared small; the bladder was large, and not far from health; the urine contained some mucus, and gave out an odour slightly fœtid. The presence of the catheter in the urethra and bladder occasioned sharp pains. I sought to diminish the irritability of the part by antiphlogistic treatment, and accordingly introduced the flexible sound to diminish the sensibility. This was kept in ten minutes. This was repeated six times; after which he did not feel any pain. On the 6th of February, 1826, M. Fechen came to my house, where he found Messrs. Desgenettes, Freyant, Orfila, Moreau, and many other distinguished practitioners. The patient was placed upon a common bed, the sacrum being elevated by a cushion. The stone was discovered by the sound, which served afterwards to make an injection of warm water into the

bladder. An instrument of three lines was introduced with great ease, the stone was seized, and broken. One of the fragments appeared to me so large that it could not pass the urethra. It was laid hold of, and afterwards crumbled by the pressure of the branches of the instrument. The patient expelled, with the injected liquid, many small fragments. This operation did not last seven minutes. Immediately after, M. Fechen returned home on foot. I had prescribed a warm bath, rest, and milk for drink During the evening he urinated much pulverized stone, and two large fragments. He had an accession of fever from the operation, on account of a long walk undertaken immediately after, and above all by half a bottle of wine, which he had drank after leaving my house. The day after, this slight accident had disappeared, and he was enabled to return to his occupation without danger. Four days after he came to my house, and was sounded in the presence of most of the persons who had assisted at the operation, and they were convinced that the cure was complete."

Second Class. When the bladder contains one or more small calculi, the "Lithotritie" is applicable, although organic changes exist. "M. Michel, aged forty-one years, experienced, during fourteen months, a sensible derangement in the functions of the urinary organs. This was attributed to catarrh of the bladder. Emollients, opiates, the remedy of Leroy, blisters, cautery, &c. were prescribed. These means produced no other effect than changing the constitution of the patient. At last the stone was detected, and he came to Paris. His complexion was pale. his figure puffed up; his urine was ammoniacal, glairy, and purulent. He had frequent desire to urinate, and with much suffering. His walk was difficult and very painful, and his urine was bloody. The catheter discovered to me many calculi. The 23d of September, 1826, I made the first trial, in the presence of M. M. St. Rome, Bancal, and Wessely. A small stone was seized, broken, and extracted. This operation presented a remarkable circumstance: the commencement of the injection caused many sharp pains. The 27th, at another sitting, the same

result: the injection did not produce the same effect. The operation was continued the 3rd of October, and terminated the 7th. On the 14th, I satisfied myself by an exploration that the cure was complete. The patient experienced nothing untoward; he had already received decided relief after the second sitting. His walk became easy; and during the intervals of the two operations, he walked many hours without fatigue. He quickly recovered his strength and flesh. He returned to Bourdeaux the 25th of October, in a condition which left nothing to desire."

The application of the "Lithotritie" to the female. The disposition of the female organs would seem to render the application of this instrument more easy than it really is. The urethra being more short and large than that in the male, would appear to need an instrument of larger diameter; but the impossibility of fixing the meatus urinarius presents difficulties for its introduction. This difficulty is in some measure surmounted by passing the finger into the vagina, which enables you to seize the stone more easily. Madame Delange, aged sixty-two years, had stone during eight months. Her sufferings, sharp and continued, had reduced her to a great degree of marasmus. Her purulent urine was passed frequently, and with great pain. These circumstances united, weighed with me for some time upon what I should decide. I could not advise her to the common operation. If the stone was not removed, she was condemned to certain death, and that not distant. I explored the bladder, and discovered a small stone, which appeared friable. I then determined to make a trial. The preparatory treatment was short. The 23d of August, 1824, I made the first attempt. The introduction of the instrument was not so easy as I supposed. Instead of passing into the meatus urinarius, it slipped into the vagina. Having introduced it into the bladder, I seized the stone, without pain, of the size of a nut, but so friable that it was divided by the instrument. The patient passed many fragments. The operation was quick, and not painful. Five days after, in the presence of Professor Richerand, I extracted the last fragment, which

completed the cure. The general health improved more than could be hoped from the great age and infirmities of the patient.

The Third Class. "Though such happy results are obtained from the "Lithotritie," it is acknowledged that this method is not yet applicable to all cases of calculi in the bladder. When the patient is resigned in supporting, for a long time, the pains of the stone, these foreign bodies acquire in the bladder a great developement." In relating two cases of M. Civiale, we shall see wherein the instrument is not applicable. "M. Farder, aged sixty-eight years, experienced, during a few months only, some of the symptoms of stone. The desire to urinate became more frequent every day: the quantity of the liquid diminished in proportion, but the patient did not perceive any pain in the gland. He felt in the perineal region a weight, which increased by walking. These symptoms had been preceded by some pains in the region of the kidney, but they were so slight that he gave himself no concern about them. A few days after his admission into the Hospital, he gave up those exercises which were incompatible with his health. The 15th of November, I ascertained that there was stone, which appeared to me not large, and susceptible of being broken down; but the urethra and bladder was in a great state of irritation. He had an engorgement of the testicle of the right side: the functions appeared disturbed. The operation was postponed. During eight days he was undergoing a proper medical treatment, and flexible sounds were passed into the urethra, to diminish the irritability. The 21st he had an accession of fever: a diarrhœa, which he had for four months, occasioned alarm. The fever increased, and on the 24th the patient died. On examination, the walls of the bladder were found a little thickened, and the mucous membrane of this viscus phlogosed; a calculus, of the size of a pigeon's egg, slightly flattened, in it. The right kidney very large, containing many abscesses, and a calculus of the size of a bean. The left kidney was not far from natural; it had a small stone. The intestinal canal showed some traces of inflammation."

Case second. M. Gobert had stone a long time. The pains were sharp. However, the bladder did not appear to have suffered any sensible alteration. On the 1st of March, 1826, I ascertained, by exploration, that the bladder contained many stones. It was impossible to determine the duration of the treatment. The patient was very irritable, and the "catheterisme" was painful. These circumstances, joined to that of his health, made me renounce lithotritie, and advise cystotomie. After some months he submitted to it: seven stones were extracted, and the operation was attended with success. M. Civiale very candidly acknowledges these cases as inapplicable to his method. In the case of M. Paille, he ascertained the existence of several stones; and after destroying some of them, he advised cystotomy, and the high operation was performed, because he found the strength of the patient failing from prolonged sitting. It appears then, that the principal objections to this operation are, *the number of calculi, enlargement of the prostate*, and the *irritability of the patient.* M. Civiale observes, that "instead of yielding to medical treatment, they acquired fresh power by it."

The author next proceeds to consider the objections to his method which have been made, such as stricture of the urethra, engorgement of the prostate, &c., which he has most satisfactorily met and overthrown. It is not necessary to enter into a detail of these objections; they will be found mostly answered in the preceding considerations. When there is stricture of the urethra, this must be overcome by the usual means, and which does not in any ways interfere with the operation. It was supposed to be impossible to free the bladder of all the fragments of stone; but during the three years he has operated, M. Civiale has kept a register of his cases, and in no one instance has the disease returned. When opportunities for autopsic examination have occurred in patients who have died of disease not consequent upon the operation, the bladder has been found free from calculi. This certainly speaks much for the power of exploration of the instrument, and gives an assurance most satisfactory that there is nothing left for a reproduction of the disease. The various cases

furnished by the author are very favourable, indeed conclusive in his behalf.

M. Civiale has operated upon a boy of thirteen, who had suffered since he was seven years of age. He was subject to nephritic attacks, and other symptoms common to his disease; and although many circumstances occurred to retard his efforts, they were crowned with success, with very little suffering to the patient.

Chapter X. *On Stricture of the Urethra.* There is, perhaps, no part of surgery which has occupied so much time unprofitably as this subject. Volume after volume has fallen from the press to spread and increase the confusion which already exists. This diversity of opinion must be imputed to incorrect anatomy, and consequently false premises assumed. The diagnosis is difficult, and requires close inspection; and often, when an opinion is assumed, the ignorance of the surgeon produces a stricture by his bougie, if none existed previously. The bougie, catheter, and caustic, are powerful remedies in able hands; but we think that Sir Everard Home has been the means of creating more strictures than he has cured. But let us hear M. Civiale on the subject. He divides stricture into Spasmodic and Permanent.

First—*Spasmodic.* This form is easy to subdue, by emollients, local bleeding, cold applications, the catheter used gently, or a large sound, or with caustic cautiously applied. The cause or intensity of the disease will direct his choice in their adoption. He says excess of venery frequently occasions it. He gives the following case. "M. N., aged thirty, had many attacks of urethritis, for which he had only employed antiphlogistic treatment. He had a slight running, which increased when he lived freely. Moderate coition produced but a slight difficulty of urination for some hours. Every time it was repeated, especially when it was preceded by much erection, a complete retention of urine produced twice most alarming symptoms, which were relieved by the catheter. When he abstained, his urine flowed in full stream."

Second—*Permanent Stricture.* M. Civiale says this generally proceeds from a chronic inflammation of the urethra;

sometimes it arises from spasm; but most commonly they come on very imperceptibly. After detailing the causes, too well known to be described, he proceeds to the means of cure. The author gives a history of these means, beginning with dilatation by bougies, sounds, &c., to the consideration of his two methods, dilatation and cauterization. After freely criticising the surgeons of France and England, and pointing out their defects, and speaking of the action of caustic in the urethra, the author gives the following directions in those cases where the stricture has been of long standing, and is callous, and requires many applications of the caustic.

It requires a great many applications of the caustic to overcome a stricture, which is callous and of long standing. The instruments which he uses for this purpose are as follows. First, the conductor with its bulbous extremity, graduated to ascertain the stricture. This is hollow, and carries the caustic upon a tube, flexible or not, as the case may demand. The caustic is contained within a portion of the bougie, which is cut about four lines from the end of the bougie, and as much above it, fitting into both ends by means of screws. After he has applied the caustic, he uses dilatation, and relies altogether upon these two means for a cure. Where a new passage is to be formed altogether, he gives some salutary cautions to the inexperienced. While there is nothing very important in these improvements, all his observations are joined with such precautions, as a man of experience finds necessary to use in these delicate operations.

We cannot rise from the perusal of M. Civiale's work, without acknowledging the important service which, in our opinion, the author has rendered to surgery. He has shortened human suffering in two ways; first, by rendering the operation of lithotomy unnecessary in many cases; and, secondly, in wresting the knife from those pretenders, who seek to debase their science into a mechanical art, and elevate their character and reputation upon the ruins of medical surgery.

QUARTERLY HISTORY

OF

IMPROVEMENTS IN MEDICINE AND SURGERY.

Foreign.

PHYSIOLOGY.

On the action of carbonic acid gas in producing Asphyxia.—M. Collard, in a memoir read before the Royal Academy of Sciences, on the *action of carbonic acid gas in producing asphyxia*, arrives at the following conclusions.

1. That animals plunged into carbonic acid gas, do not die solely from the absence of respirable air, as occurs with hydrogen and azote.

2. That the experiments on which modern authors (and amongst others Bichat and Nysten) found an opposite opinion, are either inconclusive, or imperfect in several points.

3. That carbonic acid gas exercises a peculiar deleterious action, and that this action affects principally the nervous system of the brain.

4. That the most promising mode of recovery from these accidents, will probably be the excitement of the diaphragm, through the agency of the galvanic pile.—*Bulletin des Sciences Medicales.*

On the Chemical Process performed in Respiration.—The opinion sustained by Allen and Pepys, and by others, that the blood does not receive oxygen from the air, but that on the contrary, the oxygen serves only to form carbonic acid which is respired, is combatted by Professor Mayer, of Bonn. He has several times repeated an experiment by which he proves, that the walls of the pulmonary vessels are really permeable to oxygen; and that this gas combines itself with the blood. The experiment consists in killing an animal by strangulation, immediately opening the thorax and pericardium, dividing the aorta and the pulmonary artery, and injecting distilled water into the latter until all the blood of the lungs is drawn off, and the water returns quite clear through the portion of the aorta yet attached to the heart. This preparation being made, a green solution of

cameleon mineral, which has been carefully kept from the air, is injected into the pulmonary artery: it returns by the aorta, yet remaining the same colour; but if the aorta is tied, and a fresh portion of the same solution is injected, and then the pulmonary artery tied, and air blown into the lungs, and artificial respiration kept up for some minutes, the liquid injected into the pulmonary vessels will soon acquire a beautiful red colour: a proof that the oxygen of the air has acted upon the cameleon mineral by penetrating the organic walls of the vessels which contained it.

The *cameleon mineral* employed in the above experiment in a combination of the peroxide of manganese with potash or soda, effected by the absorption of a portion of oxygen; it is either a manganesiate of potash, or a compound of peroxide of manganese and potassium, according to that of the two oxides with which the excess of oxygen unites itself, which is not yet ascertained. Its name is derived from the changes of colour it undergoes when its solution is treated with acids or with alkalies.—*Lond. Med. Rep. and Review.*

PATHOLOGY AND THERAPEUTICS.

Mollescence of the Lungs.—In the January Number of our respected Northern contemporary, the Edinburgh Journal of Medical Science, Dr. Hastings of Worcester, has described a morbid state of lungs which does not exactly correspond with any that we have met with among modern pathological investigations. This is a peculiar softening of the pulmonary texture, without any tubercular disorganization, or any of the usual effects of inflammation. In consequence of this mollescence, profuse hæmorrhages sometimes occur—sometimes large ulcerous cavities are formed, not tuberculous, nor surrounded by any hardening of the parenchyma, as in hepatization. Sometimes the cellular tissue of the lung passes into a pulpy state, and makes its way through the bronchia by expectoration, without any ulcerated cavity being formed This pathological condition has presented itself to our author in very different states of the constitution:—sometimes in connexion with fever—sometimes with gradual failure of the vital powers, and slow emaciation—but, in most instances, without symptoms indicative of much mischief in the respiratory organ. Its tendency, in all cases, is to unfit the lungs for their office, and consequently to destroy life. Six cases in illustration are detailed by our author, of which we shall notice one or two.

Case 1. This was one of fever, in a man, 24 years of age, and who was not seen till the stage of collapse had taken place, with petechiæ on the surface. He seemed, however, to be recovering; but on the twenty-seventh day of his illness, there came on diffi. ulty of breathing, &c. and he died on the thirty-first day of the disease.

The lungs did not collapse when the sternum was raised. They were exceedingly soft in texture, "and very much resembled the spleen when in a very soft state." In many places there was a dark bloody fluid between

the pleura pulmonalis and the parenchyma. The trachea was found full of a bloody serous fluid, as were the ramifications of the bronchia. In the air-cells the membranous appearance was lost, and only a soft pulpy matter was discoverable.

We shall be able to notice only another case, which is an interesting one.

Case 2. A young lady had been in a dubious state for two months, in consequence of cough, some trifling expectoration in the mornings, and occasional tinges of blood in the sputa. Three of the same family had died of consumption. The young lady, however, was "full of flesh," and was gaining rather than losing in that respect. Still she had an unhealthy aspect—the pulse about 76—the tongue clean—bowels regular—breathing free—catamenia regular. In short, she complained of nothing except a dry hacking cough on first getting up in the morning. On percussion, the chest emitted a natural sound on each side, and the respiration was heard over both sides. Perfect pectoriloquism was heard on the left side, between the second and third, and third and fourth ribs. Our author therefore concluded that there was a cavity in the superior lobe of the left lung. It was considered to be of small extent, and therefore recovery possible—and, at all events, danger not imminent. Cupping between the shoulders was prescribed, and infusion of roses with tincture of digitalis every four hours. Mild unirritating diet. In a few days the blood disappeared from the expectoration, and the young lady considered herself as "in perfect health," except a trifling cough. Another examination with the stethoscope confirmed the former diagnosis of an excavation in the lungs. She went on for about a month pretty well, when one morning she brought up some blood, accompanied by a small quantity of purulent matter. Alum was added to the mixture above-mentioned, and the pil. aloes cum myrrha prescribed. The hæmoptysis proceeded no farther, and again she thought herself getting quite well. In ten days more, some hectic fever showed itself, with some ill-looking expectoration. In three days from this period there was expuition of scarlet-coloured blood. She complained of pain between the shoulders. Quietude—alum, infusion of roses, tincture of digitalis—cupped between the shoulders. The hæmoptysis continued the next day, and a grain of superacetate of lead, with a quarter of a grain of opium, every two hours, were ordered. These means proved inefficient, and she died in a few days.

On removing the lungs, they were found slightly adherent on the right side, and more so on the left. There was no appearance of purulent secretion in the trachea. The bronchia contained some sero-purulent fluid. On tracing the ramifications in the right lobes, the only appearances were, a rather suffused state of the blood-vessels in some parts—the lungs of a soft structure—and the air-vessels pervious throughout. This state obtained in the left side; "but on the upper and outer surface of the superior lobe appeared a cavity large enough to admit the half of a middle-sized orange, which had been prevented from opening into the cavity of the chest by the

adhesion of the surface of the lung to the costal pleura; but it communicated freely with the bronchial tube." Pus was searched for, but could not be found; nor was there the smallest degree of hardness in the sides of this ulcer, indicative of the process of adhesive inflammation having been set up. On the contrary, the appearance was that of simple ulcerative absorption.

Dr. Hastings has appended some judicious reflections to this and the other cases, for which we must refer to the original paper. We shall, however, quote one passage, as it bears upon a topic which we have once or twice introduced into this journal.

"May not we also take advantage of such cases as the above, and by them enforce the necessity of treating certain varieties of pulmonary consumption by a strictly tonic plan? And may not this plan, so much reprobated by some authors, as not according with the results of examination after death, be found consistent with, or at least not opposed to, the evidence of morbid anatomy?"

The organic changes which the lungs undergo in disease are extremely numerous, and many of them are quite insusceptible of diagnosis, by the most accurate pathologist, during life. A man lately came into St. George's Hospital, with the *left* side of the thorax somewhat bulged out, and completely devoid of sound or respiratory murmur, on percussion and auscultation. In the *right* side of the chest, the heart was seen and felt beating; but respiration could not there be heard, except high up near the clavicle, and backwards near the spine. The respiration was laborious, the lips blue, the pulse quick. No accurate history of the complaint could be obtained. The patient stated that he had been ill six weeks, with pain in the lower part of the sternum, cough, but no expectoration. It was pretty evident that there was fluid in the left side of the thorax, and as it might possibly be purulent, it was agreed, in consultation, that an opening should be made between the sixth and seventh ribs. This was done by Mr. Brodie, when a considerable quantity of sero-sanguineous fluid issued. The patient experienced great relief, and slept for some hours uninterruptedly after the operation. He gradually sank, however, and expired in a day or two. After the operation, the sound immediately returned in the right side, where the pulsation of the heart had previously been felt. The *left* side sounded hollow, of course, from the unavoidable admission of some air during the operation. On dissection, we understood that a large fungus hæmatodes had occupied and disorganized the left lung, as well as the corresponding portion of diaphragm, descending to the spleen.

We think the physician (Dr. Hewett) who recommended, and the surgeon (Mr. Brodie) who performed the operation, were perfectly justifiable. The man was suffering great distress in breathing, and no one could tell what was the precise nature of the fluid in the chest. Its removal, therefore, was desirable, whatever might be its nature. The great relief which followed did more than compensate for the pain of the operation, which last was justifiable on the principle of Euthanasia.—*Johnson's Quar. Jour.*

Acute Duodenitis—Encephalitis.—The following case is related by Dr. C. F. Bellingheri of Turin. A female, of robust constitution, and previously enjoying good health, began, towards the end of July, to feel loss of appetite, and depression of spirits. On the 25th August she experienced aversion towards food, and inclination to vomit—tongue foul—pain in the head—no fever. 26th. Took two ounces of manna, which she vomited. The symptoms continued, and a physician prescribed an emetic of ipecacuan and tartarised antimony. Vomiting followed, but nothing particular was discharged from the stomach—the bowels were then acted on. In the evening of this day Dr. B. saw the patient, and found her with fever; loaded tongue, the point of this organ being red; thirst great; inclination to vomit; no pain, even on pressure, in any part of the abdomen. At this period there prevailed an inflammatory fever in Florence, accompanied by considerable determination to the mucous membrane of the bowels; and, as it was suspected that this patient had made rather too free with strong liquors, Dr. B. did not hesitate to consider the disease as partaking of the reigning epidemic, and as consisting chiefly of inflammation of the stomach and bowels, which would soon affect the head. A copious bleeding was prescribed. The blood was not inflamed. Tamarind water, with sugar, for drink. 27th. The head-ache had disappeared—thirst continues, vomiting is still distressing—tongue very foul and yellow—sense of taste much depraved—great aversion to food—the food, when swallowed, produced no pain, or heat, or weight in the stomach, nor any increase of the sickness—neither was there any tenderness, on pressure, in any part of the abdomen. The motions were liquid and yellow—urine red, with a sediment. The fever continues, the pulse being quick and concentrated. Another venesection, to the extent of 16 ounces—blood inflamed. Vegetable acid drinks continued. 28th. Same symptoms: numerous leeches to the anus, followed by syncope. 29th. The thirst urgent—tongue thickly coated, red at the tip and sides—frequent efforts to vomit—no pain in the abdomen—stools abundant, liquid, and yellow—less fever to-day. Ten grains of ipecacuanha exhibited, which was soon returned, merely with the fluids drunk. Things continued in this state till the 1st of September, when the vomiting was almost incessant, and the fever became more intense than before. In the night delirium came on, but she did not complain of pain in the head—cough, and crude mucous expectoration. 2nd September—the venesection repeated to 16 ounces—blood buffed, and very consistent. 3rd. The delirium has ceased—no pain in the head—none in the abdomen, even on pressure. In the evening, an exacerbation of the fever, and the bleeding was repeated. The blood was now natural, but thick. 4th. The vomiting less frequent—the tongue begins to clean—dejections liquid and black—some miliary eruptions on the skin—general perspiration—urine clear and abundant—pulse rapid, but non-resisting—the patient could give no account of her feelings. These symptoms continued till the 7th, when the head evidently was much affected, and numerous leeches were applied there, while blood was taken from the jugular vein. Blisters, sinapisms,

&c. were employed. 8th. Bled from the feet, and ice applied to the head. 9th. Comatose—paralysis of the left arm, with convulsive contractions of the right. There was now pain, on pressure, in the region of the duodenum, as evinced by signs, rather than words. 10th. Apoplexy closed the scene.

On Dissection, the duodenum was found highly inflamed throughout its whole extent, being of a deep brown colour, and adherent, by means of coagulable lymph, to the neighbouring parts. The stomach was also inflamed, in the vicinity of the pyloric orifice. The rest of the intestinal tube was sound, as were the liver, and other abdominal viscera. In the head, the pia mater was found to be inflamed and injected, particularly over the right hemisphere. The cortical substance of the brain was of a deep ash-colour, and softened. There were more than two ounces of water in the lateral ventricles, of a sero-sanguineous character. "The dissection," says Dr. B. "proves that the disease was a severe acute duodenitis, followed by encephalitis and acute hydrocephalus."

The conclusion which Dr. Bellingheri draws appears to be legitimate, and the case is curious, as showing that so sensitive an organ as the duodenum may be inflamed, and no pain even on pressure, may be complained of. The emetics in this case, very probably aggravated the disease. The extension of the inflammation, from the intestinal canal to the brain, is here finely illustrated.—*Ibid.*

Benefit of the Climate of Madeira in Phthisis.—The following table, limited as it is, will give a tolerably correct idea of what is to be expected, from a residence here, in lung cases. It is taken from those of which I happen to have memoranda, and which form a part of those which have been sent here during the course of the last eight years. It does not include the invalids who have come out this winter, many of whom will never see their native shores again.

In the cases marked "confirmed phthisis," there were copious purulent expectoration, diarrhœa, &c. and almost all of them terminated fatally here. I examined the bodies of fifteen of them after death in the presence of some of my professional friends; and in every instance the lungs were found almost completely disorganised. The extent to which the process of disorganisation may proceed before death, is better exemplified here than in Britain, as the patient's progress, *a son heure supreme*, is less liable to be hastened by accidental inflammatory attacks. In some of them the pulmonary symptoms were stated to be merely secondary, and the liver was denounced as the offender in chief; but in only *one* instance (that of a gentleman from Scotland, in whom that organ was found enormously enlarged, and of which there was sufficient evidence before death) was there found the slightest deviation from healthy structure in any of the other cavities. From this I exclude intestinal ulcerations, which are so generally met with whenever the disease has had any opportunity of running its victim completely down.

Some of those marked "incipient phthisis" were probably not fully entitled to an appellation so ominous. Their general character was young people who were said to have "overgrown themselves," and who had been subject in England to inflammatory attacks, having cough, &c. Others had suffered from neglected or mistreated inflammation, and in many there was a stroug family predisposition to pulmonary disease. Most of them, I have little doubt, would now have been in their graves but for the precautionary measure which was adopted.

Cases of Confirmed Phthisis, - - - - -		47
Of these there died here within six months after their arrival,	32	
went home in summer and returned and died,	6	
left the island, but of whose death we have heard,	6	
and not since heard of (probably dead)	3	
Cases of Incipient Phthisis, - - - - -		35
Of these there left the island, much improved in health, and of whom we have heard good accounts, - -	26	
also improved, but not since heard of, -	5	
and have since died, - - - -	4	
Other diseases, - - - - - -		15
		97

From this it appears, that there are in England two sorts of cases in which transportation is recommended; those which are curable, and principally those which are not. Regarding the latter order, I shall merely observe that, in cases of the common tubercular phthisis, in which suppuration has commenced, a prolongation of existence, (and that under severe restrictions) is all that a residence here can be expected to afford. When it has proceeded to a considerable extent, I should consider it the duty of a medical attendant, not only not to advise the adoption of such a measure, but most earnestly to dissuade from it those who, from hearsay evidence of the recovery of others in circumstances similar to their own, may feel disposed to fly to it as a last resource.

That great and lasting benefit is to be derived from even a temporary residence in this climate, which is probably inferior to no other, in cases where pulmonary disease is merely threatened, or where strong family predisposition to it exists, many living examples sufficiently prove. But, even under such comparatively favourable circumstances, it ought to be strongly impressed on the mind of the invalid, that half measures are worse than useless; and that no advantage is to be derived from climate, however fine, unless it be steadily seconded by the utmost caution and prudence on his part.—Madeira, 18th Nov. 1826. *From a paper by Dr. Renton, in the Edinburgh Medical and Surgical Journal for April.*

Small-pox.—Hufeland has lately published in his *Journal der Practischen Heilkunde*, some observations on the epidemic small-pox of 1823 and 1824, with results deduced from facts and observations relative to the modifying forms of that disease.

The following are among the most interesting.

1. Small-pox is not reproduced anew, either in the atmosphere, nor organically (*dans l'organisme*), but is always caused by a contiguous principle, communicated mediately or immediately from one individual to another.

2. Small-pox is not propagated through the medium of the atmosphere; on the contrary, the contagious principle is weakened by its diffusion through it. Its contagion is communicated either from contact with a diseased individual, or with a solid body impregnated with the poison. The atmosphere surrounding a diseased person is contagious only from its holding a large quantity of vapour, charged with the variolous principle that exhales from the sick, and which mixes itself mechanically with the air.

3. When small-pox becomes epidemic, we are not to refer to the atmosphere as the producing cause, that can only favour its extension in this instance. It is always the result of the communication of the contagious principle from one individual to another.—*Bulletin des Sciences Medicales.*

SURGERY.

Directions for using the Lunar Caustic. By John Higginbottom.—I am desirous of giving a distinct account of the plan which I have learnt from experience to be the best, in applying the lunar caustic in those diseases in which I have hitherto employed it; for the *proper mode* of application of the caustic is quite essential to secure its good effects, and to avoid some rather disagreeable consequences of a careless mode of using it.

In the first place, I always prefer to use the lunar caustic in its solid form; for it is in that state much more manageable than in any other. It is necessary to moisten the surface to which it is applied slightly with pure water, except in the case of ulcers from which lymph or pus exudes, and then this is only necessary in regard to the surrounding skin.

In the second place, it is essential to know the precise effects of the lunar caustic, in the different degrees of its application. If the caustic be passed once slightly over the moistened skin of any part, except the hand (upon which the cuticle is thicker than elsewhere), it induces an eschar simply; if it be passed over the surface twice or thrice, to the eschar will be added some vesication; if more frequently still, there will be vesication only. In the first case, there will be no pain; in the second and last, there will be soreness proportionate to the degree of vesication.

It is essential to the success of this plan of treatment by the lunar caustic, that these observations be kept constantly in view.

I shall now first describe the mode of application of the caustic in the treatment—

1. *Of recent bruised wounds of the shin, &c.*—In recent bruised wounds of the shin, the caustic should be applied upon the wound, taking care to leave no spot untouched, and upon the surrounding skin, to the breadth of

one-third of an inch, in such a manner as to induce an eschar without vesications. Any moisture which may remain upon the wound is then to be removed, by gently applying a little linen or lint, and the skin surrounding that to which the caustic was to be applied is to be moistened, and covered with gold-beater's skin, so that the whole may be protected from accident; the parts are then to be kept cool, free from covering, and exposed to the air.

This is usually all the treatment which is required in this kind of injury. I have generally found that an adherent eschar is formed, and that no further application or attention is required, except in old people, in whom the skin is sometimes irritable from various causes: in this case a little fluid will form upon the edges of the eschar, and will require to be evacuated by a small puncture, as in the treatment of ulcers about to be described; the goldbeater's skin being removed for this purpose, and then re-applied.

If the eschar be removed by accident, at any time, the application of the caustic must be repeated as before. If due care be taken to avoid this kind of accident, I have not, in general, found it necessary to enjoin rest.

2. *Of small ulcers.*—I have stated, in my Essay on the Application of the Lunar Caustic, what were the cases in which I supposed it was proper to use this remedy. I have, since the date of that publication, improved much upon the mode of its application, and discovered many new instances of its utility.

The treatment of ulcers by the caustic certainly requires more care and attention than some other cases; yet I have seldom found it necessary to attend daily to them for more than nine or ten days. It is of the greatest importance that the application of the caustic should be made with the utmost care; I shall, therefore, be very explicit in giving my directions for this purpose.

The surrounding skin is first to be moistened, and the caustic applied lightly, so as not to induce vesication, to the extent of half an inch round the ulcer. It is then to be applied over the ulcerated surface: and it may be applied more freely upon this surface than in the case of a recent wound. The whole is then to be protected by goldbeater's skin, in the manner already described.

The application of the caustic round the ulcer subdues the inflammation of this part, and induces a firmer, and more continuous, and adherent eschar. If any detached vesication be induced, it is to be simply exposed to the air; but if it communicate with the surface of the ulcer, the fluid is to be carefully evacuated. A light dress, as wide trowsers, if the seat of the ulcer be upon the leg, is to be worn.

On the succeeding day, the goldbeater's skin is to be removed, by being moistened with a little water: a small smooth incision is to be made, by means of a penknife, through the eschar in its central part, and then a little pressure is to be made, so as to evacuate any fluid which may have been effused; this fluid is to be carefully removed by a little soft linen; the

breach in the eschar is to be repaired by re-applying the caustic; and the whole is to be protected, as before, by goldbeater's skin.

"On the first and second days, there is usually little fluid secreted; for five or six succeeding days, rather more is formed. The same means must be employed for evacuating the fluid every day, until the eschar finally becomes completely adherent. This will be ascertained by the appearance of indentations in the surface of the eschar, and usually occurs about the tenth day. It is remarkable, that in cases in which an eschar has been formed over a slough, it has required double the number of days to become adherent.

During the unadherent state of the eschar, it is proper to administer an efficient purgative medicine every second or third day, and to enjoin rest. Afterwards, it is necessary carefully to remove the portions of the eschar as they separate at the edges, by means of a sharp pair of scissors, and to take great care to preserve it in its situation by the goldbeater's skin, and from being detached by accident.

3. *Of punctured wounds and bites.*—In *recent* punctured wounds, the orifice of the wound must be first examined: if there be any loose portion of skin closing the orifice of the wound, it is to be removed by a pair of sharp-pointed scissors, or by a lancet; the puncture and the surrounding skin are then to be moistened with a little water; the caustic is to be applied to the former until some pain be experienced, and over the latter lightly, so as not to induce vesication. The caustic is then to be applied to the skin, for an inch round the puncture, and to a greater extent if the swelling exceeds this space. The part is then to be exposed to the air.

These cases are generally adherent from the first application of the caustic, but I have sometimes found the eschar to separate from the wound before it has healed, owing to its conical form: it is then only necessary to repeat the application of the caustic slightly, to complete the cure.

At a later period of punctured wounds, inflammation is usually present, the punctured orifice is nearly closed by the swelling, and a little pus has generally formed within. A slight pressure is to be applied to evacuate this fluid; the caustic is then to be applied within the puncture, and upon and a little beyond the surrounding inflamed skin, and the parts are to be exposed to dry. In this manner an adherent eschar is formed, and the inflammation subsides. If there be any vesication, it may be simply left to nature; the fluid is soon absorbed, or evaporates.

If there be reason to suppose that an abscess has formed deeply, it must be opened freely by the lancet, and the caustic is then to be applied within the cavity; a poultice of bread and water, and cold water as a lotion, are then to be applied over the whole. The application of the caustic may be repeated every second or third day, if the swelling or inflammation require it; and the cold poultice may be renewed every eighth hour.

I have several times applied the caustic over an inflamed surface in cases in which I was not aware that suppuration had taken place. Even in these instances, an immediate check was given to the surrounding inflammation,

and relief to the pain; but two or three days afterwards, there was an increase of swelling, attended by some pain, which is not usual, except when there is matter or some extraneous body underneath. In these cases I made a free incision with the lancet, and applied the caustic and cold poultice.

4. *Of external inflammation.*—I have had many opportunities of trying the efficacy of the lunar caustic in the treatment of external inflammation, and have published some examples of this mode of cure in this Journal for May and June, 1826.

In this case it is best first to wash the part with soap and water, to remove any oily substance from the skin, and to wipe it dry; then to moisten the inflamed and surrounding skin, and to apply a long stick of caustic flat upon the moistened surfaces, taking care that not only every part of the inflamed skin be touched, but the surrounding healthy skin, to the extent of an inch, or more. The caustic must be passed over the surface twice or thrice only. The part is then to be exposed to the air to dry, and to be kept cool.

In twenty-four hours, if the caustic has been properly applied, it will be observed that the inflammation has greatly subsided, and its progress been checked; but if there be one spot left untouched, the patient complains of it. Every such spot must be touched with the caustic. At this period there is usually a little vesication, which, however, only does good, and never increases the inflammation, or induces irritation.

On the third day, there is usually more vesication, and less swelling, and the patient complains of a little pain, as of that of a blister; but, on pressure, the part has a puffy feeling, and is quite free from inflammation.

On the fourth day, the vesications are disappearing. It is best to leave them undisturbed, for the dried exudation defends the subjacent cutis.

On the fifth day, the vesicated crusts separate, leaving the subjacent parts free from soreness or inflammation. It is sometimes several days before the whole of these crusts peel off; but I believe it is best to leave them undisturbed.

In *erysipelas from wounds or ulcers*, the wound or ulcer, and the inflamed surface are to be treated by combining these modes of using the caustic.

In *inflammation of the absorbents*, the caustic is to be applied as in external inflammation, passing it along the course of the inflamed absorbents, and beyond the inflamed surface in every direction.

5. *Of Constitutional Erysipelas.*—In this affection, bleeding, emetics, and purgative medicines, are to be premised, and then the lunar caustic is to be applied in the following manner:—The caustic is to be applied over the whole inflamed surface, and beyond it, upon the surrounding skin, to a far greater extent than in phlegmon,—perhaps to the extent of two inches or more round the inflamed border of the erysipelas. Any fresh accession of erysipelas must be immediately treated in the same manner. By means of the caustic, I believe it will often be found that we have a com-

plete control over this disease. If the erysipelas be attended by vesication, the vesicles should be broken, and the part touched with the caustic; but if vesications arise from the use of the caustic, they may be allowed to remain undisturbed. When the erysipelas has affected the head, the scalp should be shaved, that there may be no impediment to the due application of the remedy.

6. *Of Phagedenic Ulcers.*—In phagedenic ulcers, the caustic is to be lightly applied to the whole ulcer, but particularly to its edges and over the surrounding skin. If the ulcer be situated on the glans penis, a little lint is to be left upon it; if on the other part, the cold poultice and lotion are to be applied.

7. *Of the Pain from applying the Lunar Caustic.*—I have never found the pain induced by the application of the caustic any barrier to its use. Patients generally suffer infinitely more from the inflammation, wound, or ulcer, treated in the ordinary way. The caustic gives a little pain at the time, but this is soon over. The ordinary mode of treatment is both more troublesome and painful, and for a much longer period. From the application of the caustic, in some painful circumstances, the patient experiences early, if not immediate relief; and perhaps sleeps for the first time, after passing many restless nights.

I have never observed the least bad consequences from the proper use of the caustic, though this, like all other remedies of great efficacy, requires to be employed with a due attention to such rules as experience teaches us to be best adapted to secure the objects which we have in view.—*London Medical and Physical Journal for April* 1827.

OBSTETRICS.

Comparative number of Male and Female Children, &c.—The following table exhibits the number of cases that have come under my care during the course of seven years, with the comparative number of male and female children, natural children, twins, forceps cases, still-births, and breech presentations.

	Sons.	Daughters.	Total.	Illegitimates.	Cases of Twins.	Forceps.	Dead born.	Breech presents.
Aug. 1818	12	14	26	2	2	2	2	
Jan. 1819	67	44	111	5	3	5	5	3
—— 1820	56	41	97	7	3	7	5	1
—— 1821	57	74	131	3		3	3	2
—— 1822	55	69	124	6	1	3	3	2
—— 1823	73	68	141	12	2	4	2	
—— 1824	88	55	143	7	1	4		1
—— 1825	68	65	133		3	9		2
	476	430	906	42	15	37	20	11

The above includes two cases in which the face presented, and one shoulder presentation. I never met with a case requiring the use of the crotchet; and it may excite some surprise, when I state that I have no such instrument in my possession.—*Edin. Med. and Surg. Jour.*

MEDICAL JURISPRUDENCE.

Memoir upon the Separation of the Umbilical Chord. By M. Billard.—In this essay, which is published in les Archives Générales de Médecine for November 1826, M. Billard has entered at some length into the history of the separation of the umbilical chord, both as regards physiology and legal medicine.

The observations from which the memoir was drawn up, were made upon eighty-six infants, of different ages, of both sexes, and apparently enjoying a good state of health.

M. Billard was first struck with the very remarkable difference that exists between the umbilical chords of different infants. Some are voluminous, soft, and may even be termed fat; an appearance owing to a superabundance of the gelatine of Wharton; others are small, thin, and contain much less of this substance. The first desiccate more slowly than the last. There is a tendency to soften, and they often suppurate, especially at their base. The latter become very soon dry, and transparent as parchment; and when dry, black lines, the remains of the obliterated vessels, are manifest on holding them to the light. The proportion between the thin chords and those which have a greater quantity of gelatine, is one-third for the first, and two-thirds for the second. Before drying, the chords wither, and this withering may be regarded as the first degree of desiccation. The following statement exhibits the different periods at which it commences:—

Of eighty-six infants that were observed, sixteen exhibited the chord a little withered, and even fresh. It was soft, blue, very flexible, filled entirely the knot of the ligature, and the cut surface was still clean. Of these sixteen, one was five hours only from birth, six a day, four two days, and four three days. The withering, therefore, varies in its commencement from five hours to three days from birth. In twenty-four infants, desiccation had begun at the end of the chord, had occupied one-half, or already extended to the abdomen. Seven of these were only one day old, eleven were two days, three were three days, and three were four days, from birth. In some, the chord was large, and very soft at its base, which was surrounded by a thick and prominent cutaneous band. In all, the cut surface of the chord was blackened, shrivelled, and was loose in the ligature. Hence it appears, that desiccation generally begins on the first or second, and yet may have made little progress on the fourth day.

The period of complete desiccation is not less variable. Twenty-five had the chord entirely dry. Five were two days old, nine three days, five five days, four four days, one one day, and one a day and a half, from birth. Three days, therefore, is the period at which desiccation is ordinarily complete; but it may be perfected as early as the first day, or may be retarded to the fifth. But it should be remarked, that in the former case the umbilical chord was very thin.

M. Billard regards the desiccation of the chord as the result of vitality. He states, that the portion of chord connected with the placenta does not undergo the same changes as that part of it which is attached to the child.

The former withers, and putrefies as dead matter: but this does not happen with the abdominal portion of the chord. The desiccation of the latter ceases the moment that life is extinct. The one is a true decomposition; the other is merely desiccation without decomposition.

This phenomenon M. Billard does not attempt to explain positively, but is disposed to attribute the desiccation to animal heat, which during the life of the child may favour evaporation; while, on the contrary, the moisture exhaled from the body after death may preserve the softness of the chord, and even favour its decomposition.

M. Billard sums up this part of his paper with the following inferences:

'1. The desiccation of the umbilical chord takes place during life only.

'2. At the moment of death this desiccation is completely suspended, or considerably diminished.

'3. If the chord is fresh, or commencing to wither, the infant may either have been born dead, or have lived only a short time.

'4. If the chord is either much or completely desiccated, the infant has at least lived one day.'

These inferences will be so much the more accurate, as the examination shall be made nearer to the period of the infant's death.—*London Medical Review.*

Domestic.

MEDICAL STATISTICS,

Or a Comparative View of the Mortality in New York. Philadelphia, Baltimore, and Boston, for a series of years; including Comparisons of the Mortality of Whites and Blacks in the two former Cities, and of Whites, Free Blacks, and Slaves, in Baltimore. By NATHANIEL NILES, jr. M.D. and JOHN D. RUSS, M. D.

[Copyright secured by the Authors.]

THE matter contained in the following Tables has been taken from authentic bills of mortality; and in as far as comparisons have been instituted, diseases of the same general character have been included under the same general head for each of the cities.

The census of 1810 and of 1820 for New-York, and of 1820 and 1825 for Boston, have served as a basis for estimating the population, on the supposition that the increase has been regularly progressive from one of those periods to the other.

In New-York, the office of Inspector is filled by a medical gentleman; to whose intelligence and care we are indebted for the greater regularity of the returns for that city, compared particularly with those for Baltimore and Boston.

Would not the ends of justice, as well as the cause of science, be promoted by the appointment of a medical officer in every city, whose duty it

should be to examine the circumstances attending the death of every individual, and to make a return of the age, sex, profession, disease and its duration, as well as the time and place of death; regard being always paid to the circumstances of colour, freedom, and slavery?

A uniform method might be adopted for reporting these facts in a tabular form, so as greatly to facilitate future investigations and comparisons. In this way the chances of life in one city could be readily compared with those in another; the comparative advantages of town and country residence, under similar latitudes determined. The influence of various causes of death, which are now unseen and unknown, could thus be measured and comprehended. But these investigations should be accompanied by topographical and meteorological observations.

The study of diseases under their general classification, in relation to topography and the influence of climate, as heat and cold, drought and dampness sunshine and shade, exposure to winds, &c. is of great interest; and these investigations become more important as our inquiries extend to the influence of occupations, poverty and wealth, political superiority or subordination, prevailing vices, &c. upon the longevity of man, his reproductive capacites, intellectual and physical perfection, under the more or less varying effects of these several causes.

A sufficient number of observations, embracing all these conditions, would afford a sure basis for a comparative estimate of the value or length of life, under the dominant influence of each of these conditions.

The combining and comparing the great facts in the natural and political history of every individual, offers a wide and unoccupied field, in this country at least, for interesting research.

The recording of all births, and the ages of all persons who marry, would furnish the means for ascertaining the duration of successive generations, and of instituting inquiries as to the influence of climate, the greater or less degree of refinement in society, &c. upon the developement of the human body into a state of perfect man or womanhood.

To pursue our remarks, we will venture to call the public attention to the importance of having annual returns made of the amount of political crimes in each State; so that they may be classified, and compared with crimes of a similar character in other States of the Union, in proportion to their population, and also with other countries.

By these means the influence of climate, laws, manners, systems of religion, occupations, slavery, &c. would be rendered comprehensible, and enable us to appreciate the perfection of our own country and institutions, in comparison with those of other countries.

Let similar investigations be instituted in relation to all the evidences of public prosperity and distress, and the whole elements of society will be disclosed to our contemplation, and lead us directly to a knowledge of the defects and their causes, in our system of social and legal order. In this extended sense, statistics furnish the true lights to the philosopher and legislator.

TABLE I.

Summary of Deaths per year, in the City of New-York, for a series of eleven years, by the following Diseases.

Years.	Population.	Whole No. of Deaths.	Consumption.	Acute Diseases of the Lungs.	Whole No. of Deaths by Diseases of Lungs.	Fevers.	Dropsies.	Dysentery.	Cholera Infantum, (for nine years.)	Croup.	Tabes Mesenterica.	Gastro Enteritis.	Whooping Cough.	Apoplexy.	Measles.	Intemperance.	Diseases of the Liver.	Palsy.	Smallpox.	Suicide.
1816	111830	2739	678	188	866	129	211	71	...	87	47	53	44	53	19	30	30	22	179	15
1817	114660	2527	574	137	711	221	187	71	...	69	39	61	11	46	20	40	54	29	14	15
1818	117560	3265	591	175	766	347	241	141	68	78	111	61	123	46	18	38	40	46	19	24
1819	120550	3176	577	126	703	286	223	219	133	78	137	47	55	67	10	35	41	40	00	27
1820	123606	3515	625	147	772	346	289	242	169	104	113	64	19	54	74	28	44	41	00	15
1821	131120	3542	715	155	870	339	287	142	114	140	96	60	92	45	109	64	45	37	00	16
1822	139100	3231	624	165	789	398	245	109	115	125	84	73	35	60	1	44	40	36	00	13
1823	147580	3444	683	181	864	190	291	98	150	102	93	88	31	58	117	43	48	31	18	18
1824	156530	4341	736	210	946	179	377	120	102	154	120	123	116	52	100	70	50	40	394	19
1825	166087	5018	843	295	1138	435	343	138	151	141	143	117	69	147	53	84	72	62	40	14
1826	176190	4973	820	290	1110	335	350	193	222	161	132	114	126	69	31	55	64	44	58	29

In this Table, under the head Acute Diseases of the Lungs, we have included Pleurisy; for although in its commencement it may be a distinct disease, it rarely, if ever occasions death, without affecting the body of the lungs.

Every description of Fever as returned, except Scarlet and Hectic, are included under the general head of Fevers. The Typhoid character of Puerperal Fever appears so evident from its general prevalence at the same epochs with Typhus, that we have taken the liberty to include it also under the same head.

Under Dropsies, all forms of that disease are included.

TABLE II.

Summary of Deaths per Month in the City of New-York, for a series of eleven years, by the following Diseases, viz:

DISEASES.	January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.	Total.
Consumption	660	659	644	664	616	510	591	663	579	585	645	650	7466
Acute Diseases of Lungs	200	292	254	230	210	114	117	105	92	141	152	172	2069
Fevers	178	137	158	186	211	245	262	374	497	441	305	211	3205
Dropsies	244	257	241	245	259	242	272	308	269	259	210	245	3044
Dysentery	22	14	29	7	16	28	233	489	378	203	79	36	1544
Cholera Infantum(10y'rs)	2	2	2	1	5	32	246	527	288	108	14	18	1245
Croup	137	106	122	106	91	66	69	67	84	140	139	122	1239
Tabes Mesenterica	63	66	85	95	79	69	80	126	139	135	89	84	1115
Gastro Enteritis	48	54	56	60	60	76	121	97	94	69	67	59	861
Whooping Cough	53	66	42	33	37	31	73	105	105	71	56	49	721
Apoplexy	49	59	56	47	57	45	109	59	45	59	55	57	697
Measles	48	42	46	45	32	44	65	77	45	39	24	45	552
Intemperance	49	32	33	31	49	47	46	43	44	47	63	47	531
Diseases of the Liver	48	46	34	35	34	39	57	44	36	64	41	50	528
Palsy	42	42	44	31	28	28	31	38	25	41	34	44	428

This Table presents an interesting view of the influence of season upon different diseases. Consumptions, Dropsies, Diseases of the Liver, and Palsy, being chronic forms of disease, are not, in their termination, particularly under the influence of season: On the contrary, its effect is strikingly manifested in all acute forms of disease.

Table III.

Table showing the proportion of Deaths in New-York, by the following Diseases, to the whole number of Deaths, and the proportion of Deaths (stillborn excluded) to the Population.

Years.	Population.	Whole No. of Deaths, (stillborn excluded.)	Deaths to Population, (stillborn excluded) as one in	By Consumption, as one in	By Acute Diseases of the Lungs, as one in	By Diseases of the Lungs, as one in	By Fevers, as one in	By Dropsies, as one in	By Dysentery, as one in	By Cholera Infantum, as one in	By Croup, as one in	By Apoplexy, as one in	By Intemperance, as one in	By Palsy, as one in
1816	111830	2651	42 18	3 91	14.10	3.06	20.55	12.97	37 33	.....	30.47	50.01	88.36	120.50
1817	114660	2409	47.59	4 19	17 58	3.38	10 90	12 8[illegible]	33 9[illegible]	.....	34 92	52 37	60.22	83.06
1818	117560	3106	37 84	5.26	17 74	4.05	8.95	12.88	22.02	45 67	39.81	67.52	81 74	67.52
1819	120550	3008	40 07	5.21	23.89	4 28	10 51	13.4[illegible]	13 73	22.62	38 56	44.89	85 94	75.20
1820	123606	3326	37 10	5 32	22.62	4 30	9 61	11.50	13.71	19 68	31 98	61 59	118.78	81.12
1821	131120	3368	38.93	4 71	21.72	3.87	9 93	11 73	23 71	29.54	24 05	74.84	52 62	91.02
1822	139100	2974	46.77	4 76	18.02	3 77	7 47	12 14	27.28	25.89	23 79	49.56	67.59	82.62
1823	147580	3221	45.81	4 71	17 79	3.73	16 95	11.07	32 86	21.47	31.57	55 53	74 90	103.90
1824	156530	4091	38.26	5 55	19.48	4 32	22 86	10.85	34.09	40 10	26 56	78.67	58.44	102.27
1825	166087	4774	34 79	5.66	16 18	4 19	10.97	13.91	34 59	31 61	33 86	32.47	56.83	77 00
1826	176190	4671	37 71	5.69	16 10	4.20	13 94	13.34	24.20	21.04	29 01	67.69	84.92	106.15

The Census of 1810, 1820, and 1825, have served us as a basis for estimating the population of the intermediate years, on the supposition that the increase has been regularly progressive from one of those epochs to the other.

Table IV.

This Table shows the proportion of Deaths, among the Blacks, to their Population, in the City of New-York for the following years.

Years.	Black Population.	Whole Number of Deaths.	Proportion of Deaths among the Blacks, to Population is as 1 in
1821	10730	550	19.50
1822	.....	*	.....
1823	11600	432	26.85
1824	12070	718	16.81
1825	12559	875	14.35
1826	13060	743	17.53

It is worthy of remark, that the rate of increase among the Blacks of the city of New-York, is about 2 per cent. less than that of the Whites, for the last eight years. It is to be regretted that the *Diseases* which have swept off the *Blacks* in such disproportionate numbers, have not been reported.

* The Deaths were not returned for the year 1822.

Table V.

Summary of Deaths in the City of Philadelphia, for a series of seven years, by the following Diseases, viz.

Years.	Population.	Whole Number of Deaths.	Consumption.	Acute Diseases of the Lungs.	Whole Number of Deaths by Diseases of the Lungs.	Fevers.	Dropsies.	Measles.	Intemperance.	Diseases of Liver.	Whooping Cough.	Palsy.	Smallpox.	Suicide.
1820	114410	3274	446	141	586	525	209	47	21	35	11	36	00	4
1821	116810	3172	438	134	572	402	194	00	45	49	36	40	00	7
1822	119260	3591	488	124	612	498	243	00	66	36	38	28	00	6
1823	121760	4600	536	141	677	744	241	156	6[illegible]	42	79	39	16[illegible]	6
1824	124320	4399	576	178	754	647	221	102	75	41	42	47	324	2
1825	126930	3812	519	145	664	362	270	38	95	46	40	42	6	9
1826	129600	4151	587	222	809	421	242	101	51	59	43	31	3	13

Table VI.

Table showing the proportion of Deaths in Philadelphia by the following Diseases, to the whole number of Deaths—and the proportion of Deaths (stillborn excluded) to the Population.

Years.	Population.	Whole Number of Deaths (stillborn excluded.)	Deaths to Population (stillborn exc., as one in	By Consumption, as one in	By Diseases of Lungs, as one in	By Fevers, as one in	By Dropsies, as one in	By Intemperance, as one in
1820	114410	3189	35.87	7.15	5.44	6.07	15.25	151 85
1821	116810	2961	39.45	6.76	5.17	7.36	15.26	65.80
1822	119260	3334	35 77	6.83	5.44	6.69	13.72	50 51
1823	121760	4372	27.84	8.15	6.45	5 87	18.14	67 26
1824	124320	4284	29.02	7.43	5 68	6.62	19.38	57.12
1825	126930	3539	35.86	6 81	5.32	9.77	13 18	37.25
1826	129600	3845	33.70	6 55	4.75	9 13	15.38	75.39

In this Table the rate of increase, as determined by the census of 1810 and of 1820, have served as a basis for making out the population for the subsequent years, there having been no census taken in 1825.

Table VII.

Proportion of Deaths among the Blacks, in Philadelphia, to their Population.

Years.	Population.	Whole Number of Deaths.	Proportion of Deaths to Population as one in
1820	10994	. .	. . . . .
1821	11220	686	16.35
1822	11460	560	20.46
1823	11700	800	14.62
1824	11940	703	16.98
1825	12190	495	24.62
1826	12450	529	23.53

Table VIII.

Summary of Deaths per year in the City of Baltimore, for a series of eight years, by the following Diseases, viz.

Years.	Population.	Whole Number of Deaths.	Consumption.	Acute Diseases of the Lungs.	Whole No. of Deaths by Diseases of Lungs.	Fevers.	Dropsies.	Measles.	Intemperance.	Whoop'g Cough.	Diseases of Liver	Palsy.	Smallpox.	Suicide.
1819	60900	2287	258	70	328	521	68	116	46	78	. .	7	1	3
1820	62738	1625	320	42	370	158	58	00	29	27	15	17	00	4
1821	64630	2015	336	33	369	395	70	2	47	28	21	18	21	2
1822	66590	2319	296	46	342	429	88	4	47	17	48	19	122	3
1823	68600	2108	236	66	302	303	75	175	25	31	18	11	2	5
1824	70670	1468	188	47	235	185	47	14	25	14	18	9	2	5
1825	72810	1545	295	44	339	135	68	9	21	13	33	11	3	7
1826	74990	1922	306	48	354	164	84	3	21	45	29	14	4	4

Table IX.

Table showing the proportion of Deaths in Baltimore, by the following Diseases, to the whole number of Deaths—and the proportion of Deaths (stillborn excluded) to Population.

Years.	Population.	Whole Number of Deaths (stillborn excluded.)	Deaths to Population (stillborn excluded.)	By Consumption, as one in	By Diseases of Lungs, as one in	By Fevers, as one in	By Dropsies, as one in	By Intemperance as one in
1819	60900	2182	27.91	8.45	6.65	4.18	32.08	47.43
1820	62738	1527	41.08	4.65	4.12	9 66	26.32	52.65
1821	64630	1910	33.83	5.68	5.17	4.83	27.28	40.63
1822	66590	2198	30.29	7.42	6.42	5.12	24.97	46.76
1823	68600	2000	34.30	8.47	6.62	6.60	26.66	80.00
1824	70670	1366	51.74	7.26	5.81	7.38	29.06	54 64
1825	72810	1453	50.11	4.92	4.28	10.76	21.24	69.19
1826	74990	1825	41.09	5.96	5.15	11.12	21.72	86.90

Table X.

This Table shows the proportion of Deaths among the Blacks, (slaves included) and among the Slaves exclusively, in Baltimore, to their Population, for the following years.

Years.	Population of the Blacks (slaves included.)	Whole Number of Deaths among the Blacks.	Deaths among the Blacks, to their Popu. as one in	Population of the Slaves.	Whole Number of Deaths among the Slaves.	Deaths among the Slaves, to their Popu. as one in
1820	14651	488	30.02	....	..	.....
1821	15090	423	35.67	....	..	.....
1822	15550	582	26.71	....	..	.....
1823	16020	659	24.30	4760	39	66.19
1824	16510	416	39.68	4910	48	102.29
1825	17010	389	43.72	5050	57	88.73
1826	17520	526	33.32	5210	97	53.92

Table XI.

Summary of Deaths in the City of Boston, for a series of seven years, by the following Diseases, viz.

Years.	Population.	Whole Number of Deaths.	Consumption.	Acute Diseases of the Lungs.	Whole Number of Deaths by Diseases of Lungs.	Fevers.	Dropsies.	Dysentery.	Measles.	Intemperance.	Whooping Cough	Gastro Enteritis.	Palsy.	Diseases of Liver	Apoplexy.	Smallpox.	Suicide.
1820	43940	1103	220	28	248	74	14	14	00	31	24	5	16	7	10	00	6
1821	46490	1420	216	31	246	76	40	60	149	36	26	10	22	8	7	00	2
1822	49190	1203	166	46	212	57	44	31	3	25	5	5	10	7	6	00	5
1823	52050	1154	184	42	226	49	34	25	00	17	17	15	5	9	11	00	3
1824	55080	1302	244	84	328	36	59	45	2	26	13	16	12	6	9	2	5
1825	58281	1450	220	73	293	68	69	56	77	30	27	15	14	25	12	1	4
1826	61620	1254	231	49	280	60	71	47	10	43	23	25	9	17	10	00	5

Table XII.

Table showing the proportion of Deaths in Boston, by the following Diseases, to the whole number of Deaths—and the proportion of Deaths, (stillborn excluded) to the Population.

Years.	Population.	Whole Number of Deaths (stillborn excluded.)	Proportion of Deaths to Population, as one in	By Consumption, as one in	By Diseases of Lungs, as one in	By Fevers, as one in	By Dropsies, as one in	By Intemperance, as one in
1820	43940	1014	43.33	4 60	4.08	13.70	72 42	32.70
1821	46490	1319	35.24	6.10	5.34	17 35	32.97	36 66
1822	49190	1088	45.21	6.55	5.13	19.08	24.72	43.52
1823	52050	1045	49 80	5.67	4.62	21 32	30.73	61.47
1824	55080	1213	45 40	4.97	3.69	14.22	20.55	46.65
1825	58281	1362	42.79	6.19	4 64	20.03	19.73	45.40
1826	61620	1167	52.80	5.05	4.16	19.45	16.43	27.13

Table XIII.

This Table shows the relative proportion of Deaths among the Whites and Blacks, to their Population, in the Cities of New-York Philadelphia, and Baltimore.

	Years.	Deaths among *Slaves* in Baltimore, to Population (Whites and free Blacks exc.) as one in	Deaths among *Free Blacks* in Baltimore, to Pop. (Whites and Slaves exc.) as one in	Deaths among *Blacks* in Baltimore, to Population (Whites excluded) as one in	Deaths among *Whites* in Baltimore, to Population (Blacks excluded) as one in	Deaths among *Blacks* in Philadelphia, to Pop. (Whites excluded) as one in	Deaths among *Whites* in Philadelphia, to Pop. (Blacks excluded) as one in	Deaths among *Blacks* in New-York, to Pop. (Whites excluded) as one in	Deaths among *Whites* in New-York, to Pop. (Blacks excluded) as one in
	1820	... ..	.....	30.02	42.29	.....	.....	.....	.....
	1821	.....	.....	35.67	31.11	16.35	42.47	19.50	40.23
	1822	.....	.....	26 71	29.38	20.46	35.56	.....	.....
	1823	66.19	.....	24.30	36.28	14.62	28.96	26.85	45.14
	1824	102 29	31 52	39.68	51.48	16 98	30.40	16.81	39.37
	1825	88.73	36.03	43 72	48.26	24.62	34.59	14.35	37.05
	1826	53.92	28.69	33.32	41.16	23.53	32.34	17.53	38.58
Average...		77.78	32.08	33.34	39 99	19.42	34.05	19.01	40.17

This Table presents several imposing and important facts: 1st. The striking difference in the proportion of Deaths among the Whites and Blacks. 2d. The still more striking difference between those of the Free Blacks and Slaves of the city of Baltimore. 3d. The disproportionate number of deaths among the Blacks in the cities of New-York and Philadelphia, compared with Baltimore: the relative physical incapacity of Negroes to sustain the influence of a severe climate, and the increased effect of poverty to diminish the chances of life in cold climates, are the most active causes in producing this result. It is probable that the disparity of deaths among the Blacks in New-York and Philadelphia to the Whites, is owing to the effects of poverty upon that degraded race. The same results from similar causes, but to a more limited extent, have been observed between the poor and wealthy sections of the city of Paris. The disproportion of deaths between the Free Blacks and Slaves in Baltimore, is worthy of particular notice, and probably arises from the care bestowed on the slaves by their masters, their comparative temperance, and the more regular course of their lives; contrasted with the idleness, intemperance, and incapacity of the free blacks.

This comparative view of the Whites and Blacks impresses us most fully that so far as regards the condition of the slaves, their situation is by no means so deplorable as has been generally imagined. It is desirable that comparative researches should be continued and extended; and we regret that circumstances require our putting these Tables to press without being able (as we had contemplated) to submit the result of investigations in the large Southern and Western slave-holding Cities.

TABLE XIV.

Proportion of Deaths (stillborn included) to the entire Population, in New-York, Philadelphia, Baltimore, and Boston.

Years.	Deaths in New-York to Population, are as one in	Deaths in Philadelphia, to Population, are as one in	Deaths in Baltimore to Population, are as one in	Deaths in Boston, to Population, are as one in
1816	40.82	.....	.....	.....
1817	45.37	.....	.....	.....
1818	36.00	.....	.....	.....
1819	37.95	.....	26.62	.....
1820	35 16	33.90	38.60	39.83
1821	37.01	36.82	32.07	32 73
1822	43 04	33.21	28.71	40.88
1823	42.85	26.46	32.54	45.10
1824	36.05	28.26	48 14	42.30
1825	33.09	33.29	47.12	40.19
1826	35 42	31.22	39.01	49.13

TABLE XV.

This Table shows the average proportion of Deaths in every 1000 Inhabitants, for a series of years.

	By Consumption, in every 1000	By all Diseases of the Lungs, in every 1000	By Fevers, in every 1000	By Dropsies, in every 1000	By Dysentery, in every 1000
In New-York....	4.96	6.34	2.12	2.02	1.02
In Philadelphia..	4.20	5.47	4.21	1.89	....
In Baltimore	4.12	5.86	4.22	1.02	....
In Boston.....	4.03	4.99	1.25	0.902	....

TABLE XVI.

A Comparative View of Mortality in New-York, Philadelphia, Baltimore, and Boston, by the following Diseases.

	By Consumption, as one in	By Diseases of the Lungs, as one in	By Fevers, as one in	By Dropsies, as one in	By Intemperance as one in	By Palsy, as one in	Latitude. N.			Longitude. W.		
							°	′	″	°	′	″
In New-York..	5.03	3.94	11.73	12.35	70.80	87.84	40	42	20	74	0	45
In Baltimore..	6.46	5.47	6.31	25.91	55.40	136.42	39	17	0	76	36	0
In Boston.....	5.52	4.47	17.46	24.79	39.46	93.27	42	22	23	70	58	53
In Philadelphia	7.10	5.46	7.09	15 75	61.06	97 04	39	57	0	75	18	0
Average	6.03	4.83	10.65	17.20	54.58	103.64						

TABLE XVII.

Average proportion of Deaths to Population in the following Cities.

	Stillborn included, as one in	Stillborn excluded, as one in
Boston	41.26	44.66
New-York	37.83	40.02
Baltimore	35.44	37.47
Philadelphia	31.48	33.41
General Average..	36 50	38.89

TABLE XVIII.

Summary of Deaths in the following Cities for a series of seven years, ending the first of January, 1827.

	Under one year of age (stillborn exc.)	Between 1 and 2.	Between 2 and 5.	Between 5 and 10.	Between 10 and 20.	Between 20 and 30.	Between 30 and 40.	Between 40 and 50.	Between 50 and 60.	Between 60 and 70.	Between 70 and 80.	Between 80 and 90.	Between 90 and 100.	Between 100 and 110	Between 110 and 120
New-York..	5190	2568	2003	946	1261	3573	3617	2932	1847	1209	795	421	96	19	
Philadelphia	5438	2180	1907	1020	1296	3079	3186	2635	1842	1335	894	515	157	25	4
Baltimore ..	2464	967	938	553	987	1350	1654	1275	836	559	371	244	61	20	
Boston.....	1322	884	531	282	369	960	920	778	525	362	305	158	28	1	
Total	14414	6599	5379	2801	3913	8962	9377	7620	5050	3465	2365	1338	342	65	4

Case of Glossitis. By BENJAMIN I. BERRY, Student of Medicine, Lebanon, Pennsylvania.—The great unfrequency of glossitis having rendered its occurrence, as an idiopathic disease, extremely questionable, even with many authors, I deem it not inexpedient to relate the following case, which, in my opinion, is marked by symptoms exceedingly characteristic of that disease.

Mrs. B———, aged sixty years, residing about seven miles from this place, in consequence of exposure to cold, was attacked on the 8th of February last with a slight soreness of the throat, attended with some febrile symptoms. These being considered as trifling, did not receive any attention until the tongue had become so much enlarged in every direction, as to be pressed against the roof of the mouth, and projected beyond the lips, rendering deglutition and respiration both difficult and painful. The glands at the root of the tongue, those of the neck, and more especially the sublingual and sub-maxillary, were likewise enlarged. The family physician was now desired to visit her, who abstracted some blood from the arm, prescribed a saline purgative, some antimonial powders—recommended the

vol. liniment to be applied to the throat, and an appropriate regimen. Finding no amelioration of her complaint, but on the contrary, the symptoms so alarmingly aggravated, and she endangered with suffocation, that her friends and relatives despaired of her recovery, and they requested Dr. Gloninger to see her, who immediately pronounced it a case of glossitis, and urged the necessity of further depletion, the application of a blister to the throat, and a suitable gargle. The third day after his visit, he was informed that the inflammation had terminated in suppuration, and that a considerable quantity of purulent matter had been discharged through two apertures, one situated at the side of the frœnum, and the other near the base of the tongue. She was evidently much relieved, being again able to articulate distinctly, and take some liquid nourishment, which she had not done for several days previously. Her recovery, considering her age, was rapid; the tongue was soon reduced to its ordinary size, and every vestige of the complaint removed, so as to enable her, in the course of a fortnight, to resume the duties of her family.

Stricture of the nasal duct.—Numerous cases of disease of the lachrymal passages, constantly present themselves at the New-York Eye Infirmary, in every variety of form. It is known that the remedies for these diseases, as proposed by different authors, are extremely diverse, and their very variety proves how frequently each of them fails. Each plan of treatment has been carefully tried at the Infirmary, but never with uniformly satisfactory results, until within the last nine months. Within that period, every case supposed to be attended with stricture, has been treated in one mode, viz. by inserting a gold tube into the nasal duct, and healing the wound in the skin over it. Dr. Delafield has thus operated upon ten cases in succession, and thus far every one has succeeded. Dr. Rodgers has operated in the same manner upon the cases which have presented themselves to him, and with a similar result. Those who have performed Ware's operation with the nail-headed style, or endeavoured to remove the stricture with Travers' puncture probes, or again, with Scarpa, trusted to mere local applications to the eyelids, can appreciate from the freqnent failures of all these methods, the advantages of an operation so successful as that recently in use at the Infirmary. The operation as there performed, is the same as recommended by Dupuytren; the instruments being made after patterns obtained from Paris. The operation is minutely described, and plates of the instrument are given in a note to Mr. Travers' Synopsis of Diseases of the Eye, an edition of which has been published in this country. The instruments are now manufactured perfectly well in this city. The tubes are kept always on hand by F. Marquand, No. 166 Broadway, and the instrument for inserting them, is made by Pintard the cutler, in Beekman-street.

OBITUARY.

DIED on the 15th of April, 1827, in the 33d year of his age, JAMES FREEMAN DANA, M. D. Professor of Chemistry in the University of the State of New-York.

With the most painful feelings we find ourselves called upon in the discharge of our editorial duty, to announce the death of this excellent individual, and to record upon our pages a brief memorial of his history and his worth.

JAMES FREEMAN DANA was born at Amherst, Hillsborough County, New-Hampshire, on the 23d of September, 1793. His early education was received principally at the Academy of Exeter, in the same State, where, under the instruction of an excellent teacher, Dr. Benjamin Abbot, he laid the foundation of a liberal education. In 1809, he was admitted into the freshman class in Harvard University, and graduated in 1813. While in college, he held a respectable rank as a scholar, but was more particularly distinguished for an intense and absorbing pursuit of natural history and chemistry, and in the latter science, was much in advance of his classmates. It was at that time customary for some one of the senior class to be detailed to assist the chemical professor in preparing and performing his experiments. It was the fortune of young Dana to be allowed this privilege, and by it, he was introduced to the notice and intimate acquaintance of Professor Gorham. In the discharge of this duty, he discovered such a tact in the arrangement and performance of chemical experiments, and a mind so keenly intent upon chemical inquiries, that Dr. Gorham, understanding that he had made choice of medicine as a profession, invited him to become his pupil, kindly offering his aid in conducting his instruction free of expense; an act which was always held in the most grateful remembrance by our deceased friend. In 1813 he commenced his medical studies, and during the winter, with the consent of Professor Gorham to use his lecture room, he gave a public lecture on nitrous oxide, accompanying it with an exhibition of its effects. After this he soon became known as a practical chemist, and received constant applications for the analysis of minerals, and the preparation of particular tests.

At the close of the late war, the Corporation of Harvard College

having determined to increase their chemical apparatus, Dr. Dana was selected as a fit agent to proceed to London to procure the same. With how much benefit to the college, and honour to himself, he performed this trust, we need not say ; it is enough to add, that the winter after his return, he was appointed to superintend the construction of the laboratory at the college in Cambridge ; and in the following spring, it was opened for public lectures, displaying in all its parts, both in the lecture room and laboratory, a neatness and completeness not surpassed by any laboratory, and in the value and extent of apparatus, not at that time equalled in the United States. In the spring of 1817, he received the degree of Doctor in Medicine, on which occasion he wrote a Thesis " On the importance of Animal Chemistry." Not long after he received the appointment of chemical assistant in the college. In the autumn of the same year, he was invited to lecture on chemistry, in the medical institution at Dartmouth College, and in 1820 he was appointed Professor of Chemistry in that college. It was during his connexion with this institution that Dr. Dana established his reputation, not only as a skilful chemist, but as an able and accomplished lecturer. By the united labours of himself, and his distinguished associates Professors Mussey and Oliver, this excellent school of medicine was raised to a high rank among the medical institutions of our country.

In 1825 he published his " Epitome of Chemical Philosophy," in one volume octavo. Of this work we have already had occasion to speak in favourable terms, in a notice which we took of it in a previous number of this journal.* The opinion there expressed, has been confirmed by the most competent judges Professor Silliman in noticing it says, " Dr. Dana's arrangement is clear—his style neat and perspicuous—his selections judicious, and he has introduced the most important facts and observations that have been made in this country."† It was the avowed object of Dr. Dana, " to furnish a work which should be useful as a text book of the science, as it is usually taught in our academies and colleges." Previous to his death, he had the gratification of finding it adopted as such in two of our most respectable colleges, Columbia and Princeton.

In 1826, the Professorship of Chemistry in the College of Physi-

---

* No. XIX.

† American Journal of Science, Vol. II. p. 194.

cians and Surgeons of New-York became vacant, and it is no slight evidence of the well established character which Dr. Dana enjoyed, that although personally unknown in this city, and unsolicited on his part, he received from the Regents of the University an almost unanimous appointment to that distinguished station. At this period the situation of the College of Physicians and Surgeons was peculiar. By the Professors who had previously held stations in it, it had a few months before been left a wreck in fortune and in fame. Upon those who were appointed to succeed them, was imposed the high and arduous responsibility, not merely of retrieving the mischief which had been previously done, but of building up such an institution as should be worthy of the profession, and of the State. To the ability, zeal, and enthusiasm with which Dr. Dana laboured in this great cause, all who were acquainted with the affairs of the college, will bear a cheerful testimony. His colleagues, in an especial manner, feel a melancholy pleasure in acknowledging how much they owed him, not merely for the capacity and proficiency which he displayed in the conduct of the duties of his individual chair, but for the manner in which he co-operated with them in relation to the general concerns of the college.

During the session of the college, Dr. Dana delivered several popular lectures on the subject of "Electro-magnetism," before the Atheneum of this city, and at the time of his death he was engaged in a course of lectures before a popular audience, on chemistry generally. In both of these exhibitions, he greatly added to the reputation which he already enjoyed.

In the midst of all this usefulness, and of all these brightening prospects, he was taken from us by one of those mysterious dispensations, which so impressively teach us the vanity of human life, and the uncertainty of all human hopes and calculations.

Such is a simple narrative of the brief career of Professor Dana. From his earliest years he evinced a predilection for those studies, to which his maturer age was so entirely devoted; and the diligence with which he pursued them, is best shown in the number and variety of his recorded labours. So early as the year 1815, he bore away the Boylston prize, for an essay which he wrote on the "Tests of Arsenic." In the following year, he gained another prize, for an Essay "on the Composition of the Oxy-muriatic acid Gas." Several of his papers are to be found in the "New-England Journal of Medicine and Surgery," and almost every volume of the "American Journal of Science," contains some

contribution from his pen. Among these the principal are,—"An Account of the Grand Monadnock Mountain of New Hampshire."* An elaborate "Account of Iodine."†—"On a new form of the Electrical Battery."‡—"Chemical examination of the Berries of the Myrica Cerifera, or wax myrtle."§—"On the effect of Vapour on Flame."‖—"On the existence of Cantharidin in the Lytta Vittata, or Potatoe Fly."¶—"Chemical Examination of some animal products."**—"Notices on the connexion of Electricity, heat and magnetism, the preparation of Euchlorine Gas, &c."††—"Account of a Galvano-magnetic apparatus."‡‡—"On the Theory of the action of the Deutoxide of Azote, or Nitrous Gas in Eudiometry."§§—"On the ignition of Platinum."‖‖—"Remarks on Mr. Patten's Air Pump."¶¶—"Remarks on the common methods of detecting Cobalt."***—Additional "Remarks on Mr. Patten's Air Pump.†††

His knowledge of chemistry was minute and extensive, and his general views of that science were profound and philosophical. As a lecturer, he was luminous, impressive, and successful. Although destitute of the rarer gifts of eloquence, he possessed in no slight degree, the happy talent of enhancing with those who heard him, the importance of his subject, and at the same time of inspiring them with confidence in his own powers. As an experimenter he was unrivalled. No one could witness his experiments, without admiring the dexterity and unerring certainty with which his manipulations were conducted.

To his knowledge of chemistry, Dr. Dana added an extensive and practical acquaintance with *Mineralogy*. In the year 1818, in connexion with his brother Dr. Samuel L. Dana, also distinguished as a chemist, he published a work entitled, "Outlines of the Mineralogy and Geology of Boston and its vicinity," in one vol. 8vo. Several contributions on this subject may also be seen in the American Journal of Science, and his last production, published since his death, was an "Analysis of the Copper ore of Franconia, New-Hampshire, with remarks on Pyritous Copper."‡‡‡

---

* New-England Journal of Medicine and Surgery, Vol. V. p. 252.
† Ibid. p. 345. ‡ American Journal of Science, Vol. I. p. 292.
§ Ibid. 294. ‖ Ibid. p. 401. ¶ Ibid. Vol. II. p. 137.
** Ibid. Vol. IV. p. 149. †† Ibid. Vol. VI. p. 163. ‡‡ Ibid. p. 330.
§§ Ibid. Vol. VII. p. 338. ‖‖ Ibid. Vol. VIII. p. 198. ¶¶ Ibid. p. 275.
*** Ibid. p. 301. ††† Ibid. Vol. IX. p. 327.
‡‡‡ Annals of the Lyceum of Natural History of New-York, Vol. II. p. 253.

However extensive may have been his attainments, and whatever may have been his merit as a man of science, it is consoling to reflect, that these were not his highest excellencies. In his whole character he was amiable, virtuous, liberal, and independent. In the short space of time during which he resided in this city, he had won completely the confidence and affection of all who knew him, and his premature fate, touched with the deepest sympathy and the liveliest regret the whole community. As a mark of respect to his memory, he was buried at the public charge of the college, and the following respectful notice was entered upon the minutes of the Regents of the University, at a special meeting held on the 2d of July.

"The Regents have learnt with deep regret, since their last adjournment, of the death of JAMES FREEMAN DANA, M. D. Professor of Chemistry in the College of Physicians and Surgeons of New-York. Although attached to that institution for a single term only, yet sufficient evidence was given of his talents, and entire devotedness to his duties. He was confidently considered by the Regents as one who would increase the utility and extend the reputation of the college."

J. B. B.

QUARTERLY BIBLIOGRAPHICAL NOTICES.

1 *Letters to married Ladies.* By HUGH SMITH, M. D. to which is added a letter on corsets, and copious notes, by an American physician. New-York, 1827. 16mo. pp. 283.

---

2. *De la lithotritie, ou broiement de la pierre dans la vessie.* Par le DOCTEUR CIVIALE, avec cinq planches. A Paris, 1826, 8vo. pp. 254.

For a copy of this interesting work, we are indebted to our friend and fellow citizen, Dr. JOSEPH ARNOLD, at present in Europe. See review.

---

3. *The Western Medical and Physical Journal, original and eclectic.* Edited by DANIEL DRAKE, M. D. late Professor of the Theory and Practice of Medicine in Transylvania University, and GUY W. WRIGHT, M.D. No. 1. pp. 72. Cincinnati, 1827. Published monthly. In exchange.

This is the first attempt, under competent editors, to establish a medical Journal in the Western States, and we cannot but wish it every possible success. From a notice in this Journal, we are happy to learn that the work long since announced by Dr. Drake upon the diseases of the Wes-

tern States, has not been abandoned. We hope we shall be excused for urging upon this accomplished physician the completion of a work which has for a long time been anxiously looked for, and for which he is in every respect so eminently qualified.

---

4. *The Philadelphia Monthly Journal of Medicine and Surgery.* Edited by N. R. SMITH, M. D Professor of Anatomy in Jefferson College, and author of a Physiological Essay on Digestion. No. 1. pp. 56. Phil. 1827. In exchange.

Although unpretending in its form, this is decidedly a work of talent, and will not suffer by a comparison with the numerous medical journals with which our sister city is at present inundated.

---

5. *Annals of the Lyceum of Natural History of New-York.* Vol. II. No. 7 & 8. pp. 64. New-York, 1827.

---

6. *Journal Medical de la Gironde.* For Nov and Dec. 1826.

---

7. *Medico-Chirurgical Review.* No XII. Edited by JAMES JOHNSON, M. D. In exchange.

INTELLIGENCE.

UNIVERSITY OF THE STATE OF NEW-YORK.

APPOINTMENTS
BY THE REGENTS OF THE UNIVERSITY.

AT a meeting of the Regents of the University of the State of New-York, held in March 1827, JOHN DELAMATTER, M.D. was appointed *Professor of Surgery in the College of Physicians and Surgeons of the Western District*, in the room of JOSEPH WHITE, M.D. resigned.

AT a special meeting of the Regents of the University, held at the Capitol, in Albany, on Monday, the second day of July, 1827, Doctor JOHN TORREY Professor in the Military Academy at West-Point, was appointed Professor of Chemistry in the College of Physicians and Surgeons in the City of New-York, in the place of Doctor JAMES F. DANA, deceased.

The Professorship of Botany in said College, heretofore held by

Doctor JOHN B. BECK, in connexion with that of Materia Medica, having been relinquished by him, was transferred to Doctor TORREY, as Professor of Chemistry, and the Professorship of Medical Jurisprudence was assigned to Doctor BECK, in lieu of the Professorship relinquished by him. Doctor TORREY is therefore *Professor of Chemistry and Botany* and Doctor BECK of *Materia Medica and Medical Jurisprudence.*

The Regents also appointed Doctors THOMAS COCK, ANSEL W. IVES, FRANCIS U. JOHNSON, and EDWARD G. LUDLOW, all of the City of New-York, *Trustees*, and Doctor COCK, *Vice-President of the College.*

The following Regents were present at the meeting:—Honourable JOHN TAYLER, *Chancellor of the University;* Honourable NATHANIEL PITCHER, *Lieutenant Governor;* and Messrs. VAN RENSSELAER, LANSING, LIVINGSTON, BLEECKER, VAN BUREN, BUEL, VERPLANK, GREIG, MARCY, JENKINS, WENDELL, and KING.

At the anniversary meeting of the Medical Society of the city and County of New-York recently held, the following officers were elected, viz.

JOHN ONDERDONK, M. D. *President.*
THOMAS COCK, M. D. *Vice-President.*
JEREMIAH VAN RENSSELAER, M. D. *Cor. Secretary.*
JOHN I. GRAVES, M. D. *Rec. Secretary.*
DR. WILLIAM HIBBARD, *Treasurer.*

JOHN C. CHEESMAN, M. D.
JOHN KEARNY RODGERS, M. D.
FRANCIS N. JOHNSON, M. D.
D. L. M. PEIXOTTO, M. D.
E. G. LUDLOW, M. D.
} Censors.

The result of this election excited the greatest interest. The question to be decided by it was, whether the medical profession of New-York would lend itself to the support of a medical institution recently established in this city by Rutgers College in New-Jersey, for the purpose of breaking down the regular college of our own State. Dr. Hosack was held up by the partizans of Rutgers College, as the candidate for the office of President of the Medical Society. By immense majorities, Dr. Hosack and all his ticket were defeated. It would seem from this, that the profession in this city does not feel disposed to submit to the dictation of a New-Jersey corporation in relation to medical matters.

TO READERS AND CORRESPONDENTS.

1. Our next Number will contain Communications from Doctors KIRKHOFF, FARRAR, T. R. BECK, LUDLOW, STEVENS, STERLING, KIRBY, and JORDAN.

2. An interesting Paper, by Dr. S. W. MOORE, upon READ's Apparatus for Extracting Poisons from the Stomach, was received too late for insertion in the present Number. It shall appear in our next.